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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS
IN THE
SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Life and Writings of Berzelius. By M. P. LOUYET.[*]

WHENEVER an individual whose life and labours honour and ennoble humanity sinks into the grave, we cannot help feeling deep regret at the loss of so much intellectual riches. We are filled with sorrow when we reflect that the voice we were accustomed to honour will be heard by us no more, and that we shall no longer benefit by his enlightened instructions; we lament the extinction of the bright torch which guided our hesitating steps in difficult paths, and can scarcely regard with resignation this terrible proof of death to which the Creating Power has subjected us, which so infallibly brings us all, great and small, weak and powerful, to its own inexorable level.

These reflections arose in our minds three months ago, when the journals brought the sad but not unexpected news of the death of Berzelius. For several years, this calamity had been threatening us. Having suffered, on several different occasions, from attacks of apoplexy, he never completely rallied. For several months back, the half of his body may be said to have been in the grasp of death. Every courier from Stockholm might therefore be expected to announce one of the greatest losses which it has been the fate of scientific Europe to sustain. Berzelius himself was fully conscious of his condition; he did not disguise from himself that death was near; but he witnessed its approach with the calmness of a philosopher, and the faith of a Christian.

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[*] Read in the Academy of Sciences of Brussels on 16th December 1848.

The death of Berzelius has been considered by Sweden as a national grief. All the learned societies of the country, which may still be said to be new, have declared their intention of wearing mourning for two months. The Senate, the National Assembly, all the officers of state, of their own accord, joined the numerous assemblage which accompanied the remains of this incomparable chemist to their last resting-place.

To appreciate the scientific life of Berzelius, and analyse his works, which are as numerous as they are varied, would not only be a task of great difficulty, but would likewise require a considerable time. This arduous task, however, we should have ventured to undertake, if this master had not left behind him a brilliant constellation of zealous disciples, who now rank among the most celebrated names of scientific Europe, and who, no doubt, will not fail to pay this pious debt of gratitude, and fulfil the duties of friendship. My object will therefore be confined to laying before you a concise account of the course of a life as glorious as it was active and laborious.

JEAN-JACOB BERZELIUS was born on the 29th August 1779, at Vasersunda, a village near Linkœping, in the ancient province of Ostrogothia. His father was the teacher of a parish school in that place,—an employment of some consideration in Sweden. We have no information respecting Berzelius' early years; it appears to have been his father who taught him the first elements of knowledge. At the age of seventeen he entered the University of Upsal, with the intention of studying medicine. Afzelius, nephew of Bergmann, was Professor of Chemistry in that University, with Ekeberg as his assistant.

Poor as science was at this period, the lectures were not arranged in such a manner as to present the existing knowledge in a form which might enable the student to understand it readily; they were simply read, without being illustrated by experiments or demonstrations. Afzelius and Ekeberg appear to have given very little interest to their courses. A few tolerable analyses which they executed constitute their only title to the honour of having guided the first steps of the greatest chemist of the age on his first entrance into the field of science. Berzelius often referred, in his private conversations, to his first attempts in the laboratory of Upsal. He took pleasure in relating that, in order to accustom him to chemical manipulations, Afzelius first gave him sulphate of iron to calcine in a crucible, for the preparation of colcothar. "Any one may do work of this kind," said Berzelius; "and if this be the way you are to teach me, I may as well stay at home." "A little patience," replied Afzelius; "your next preparation shall be more difficult." On the next occasion he got cream of tartar to burn, in order to make potass. "I was so disgusted with this," said Berzelius, "that I swore never to ask for any further employment." However, he did not act upon this threat, but continued to frequent the laboratory. At the end of three weeks he was found there daily, although, according to the regulations, he was entitled to be there as a pupil only once a-week. Afzelius might have sent him away; yet he permitted him to come frequently, to engage in experiments, and to break not a few of his glasses. What displeased Ekeberg was, that the young Berzelius always carried on his operations in silence, never asking a single question. "I preferred," he said, "to endeavour to instruct myself by reading, meditating, and experimenting, rather than question men without experience, who gave me replies, if not evasive, at least very little satisfactory on the subject of phenomena which they had never observed."

After remaining two years at this University, Berzelius passed his examination in philosophy, and left it in 1798. We find him, the following year, assistant to a doctor who superintended the mineral waters of Medevi. To a mind so powerful as his, nothing could remain unobserved—all must become matter of research; and it was natural that these mineral waters should attract his attention. He accordingly made a complete analysis of them, which afterwards became the subject of a dissertation published in connection with Ekeberg, his last professor. This work was the first link in that long series of Memoirs which have raised his name to such a high degree of estimation.

In 1804, we again find him at Upsal; and he there obtained the title of Doctor in Medicine on the 24th of May in that same year.

At this period he published his "Physical Researches on the Effects of Galvanism on Organised Bodies." He was already so distinguished by his scientific works, that, on going to settle at Stockholm, a place was made expressly for him; he was nominated assistant to Sparman, Professor of Medicine, Botany, and Chemical Pharmacy, who had travelled with the illustrious Captain Cook. In consequence of the smallness of his income, he was obliged, at times, to practise as a medical man. On the death of Sparman in 1806, Berzelius' efforts were rewarded by the gift of the vacant chair. At this period, there were only three professors in the school of medicine, so that each of them was overburdened with courses. Afterwards, four others were established, and Berzelius could then confine himself to the teaching of chemical pharmacy. His lectures in medicine met with the greatest success, while those on chemistry were at first very little regarded. He does not appear, at first, to have risen much, in his mode of teaching, above his former masters, Afzelius and Ekeberg. In his method of instruction, he retained their vicious mode of reading his lectures; without any practical demonstrations or experiments. Being conscious of his own ability, and sensible of his profound knowledge, he was somewhat surprised to observe that he obtained little more success than the Upsal professor. These first attempts, joined to the advice given him from time to time by a learned foreigner, Dr Marcet, led him to abandon altogether this mode of teaching without experiments, which, although conformable to the precepts of the ancient logic, was directly opposed to the inductive method of the Baconian philosophy. It was necessary to create almost entirely the instruments of this important reformation. The laboratory left him by his predecessor presented numerous blanks; there was nothing in it, so to speak, which enabled him to develop the laws of chemistry and the properties of bodies by a well-arranged system of experiments. He zealously applied himself to supply what was wanting, and when he had added a series of simple experiments, easily understood to his own eloquent words, he assembled a considerable number of auditors, and his course became an object of admiration, as well as a model for the other schools of Europe.

It was in 1806 that Berzelius, in connection with Hisinger, commenced the publication of a periodical work, entitled *Memoirs relative to Physics, Chemistry, and Mineralogy*. One of the distinctive features of his scientific character, his marvellous facility and penetration as an analyst, shone most conspicuously in this collection. The number and value of the services he thus rendered to science, as well as the original spirit in which he had conceived his work on Animal Chemistry, published a short time after, induced the Royal Academy of Sweden to give an annual sum of 200 dollars to assist him in continuing his labours. In 1807, the same year in which he was named professor of medicine and pharmacy, Berzelius, in connection with other eminent men, founded the Medical Society of Sweden, an institution now in a most flourishing state, and which may be regarded as the soul of the Swedish Faculty. In 1808, being then only thirty-one years of age, he was nominated member of the Royal Academy of Sweden, and in 1810 he was elected president of this Society. Berzelius paid numerous visits to France; and in 1812 he visited London, and was worthily received by all the friends of science who could appreciate the services he had rendered to it. In 1815, the King of Sweden conferred on him the Cross of Chevalier of the Order of Wasa.

He was appointed perpetual secretary to the Academy of Sciences in 1818, and this office he retained till his death. In 1821, Berzelius became commander of the Order of Wasa, and some years afterwards he received the Grand Cross of that Order. At the coronation of Charles-Jean (in 1818) he was made a noble, and permission was, moreover, granted him to retain his name, which is contrary to Swedish custom. Berzelius was Officer of the Legion of Honour, and Chevalier of the Order of Leopold. In 1832 he abandoned the active labours of the professorship, entrusting to his pupil, Dr Mosander, the duties of a chair which he had occupied for thirty years: he could then follow his scientific researches without interruption, and he devoted almost his whole time to them. About this time Berzelius married; and, on the day of his nuptials, King Charles-Jean wrote to him a letter with his own hand, announcing that he had nominated him baron (*Freiherr*), and stating, among other things, "That Sweden and the world were the debtors of a man whose entire life had been devoted to works as useful to all as they were glorious to his native country." The directors of the iron-works of Sweden gave him a pension, in acknowledgment of the eminent services he had rendered to their branch of industry. In 1843, Berzelius had performed for a quarter of a century the duties of perpetual secretary to the Academy of Sciences. On this occasion, the members of the Society assembled at a banquet at which the Prince-Royal presided; in proposing the health of the philosopher, the prince expressed to him his personal gratitude for the instruction he had given him in his youth. From this period till his death, Berzelius occupied himself continually, and with his usual patience, with those varied researches which his sagacious mind and active imagination constantly suggested to him. His life flowed on in an equal current, and death approached with slow steps, as a messenger who regretted his errand. He was first seized with paralysis of the lower extremities, and knew that his end was approaching; but nothing could disturb the serenity of his powerful mind: he hastened to complete his earthly labours, and like a traveller whose toil is over, and who has reached the term of repose, he slept the sleep of the just, calm and tranquil as he had lived. Berzelius died on the 1st August 1848.

Such, in a few words, was the life of a man whose history henceforth belongs to that of chemistry, with which it is inseparably connected; and who, during the long period of half a century, constantly wrought with undiminished ardour, in increasing the intellectual treasure which the generations passing away bequeath to those that follow.

We have roughly sketched the career of this illustrious man; we may now be permitted to dwell, for a few minutes, on the character of the philosopher, and the merit of his productions.

What principally characterises the genius of Berzelius, and what we especially recommend to the attention of the future biographers of this great man, is his indefatigable ardour for work, and his inexhaustible patience. Those who wish to follow his steps, will do well to remark that these qualities were rather an acquisition than a natural endowment, and that they were indispensable to form the character of the greatest analyst of the age. Experiment, long-continued, with admirable skill, was the powerful lever he employed to advance science and render his name famous. A sagacity as lively as it was patient and circumspect, a remarkable clearness of apprehension, a skill, precision, and accuracy of manipulation in experimenting, gave to the practical results he obtained a character of certainty universally acknowledged by the learned world.

Independently of his own personal discoveries, which are numerous, and of his theories, almost equal in amount, not an experiment of any importance was made in Europe for forty years, which was not repeated, confirmed, rectified, or combated by him. In the eyes of many of the learned, Berzelius may perhaps appear of inferior rank when compared with the originators of certain general ideas, bold theories, and vast relations, which, like the world, comprehend every thing within them, but he will be placed at the summit, among the most illustrious, when judged of according to the immense number and value of the positive facts with which his perseverance and penetration have enriched science. If we take a glance at the works which he published, we will find a proof of the perpetual activity which he exerted in his laboratory or in his cabinet. The periodical work of which we have already spoken, extends over a period of twelve years, and contains a condensed view of forty-seven original researches made by himself. His great Treatise on Chemistry, in eight volumes, which has gone through five editions, rewritten almost as many times, is a monument of research and skill. Besides, Berzelius commenced in 1822, at the request of the Stockholm Academy of Sciences, an *Annual*

Report on the Progress of Physics, Chemistry, and Mineralogy, which he continued to the last, and which constitutes the most valuable collection of chemical discoveries existing in any language. We shall again revert to this work, remarkable in so many respects. With respect to his chemical discoveries, it is sufficient to mention the titles of the most important. Simple bodies,—thorinum, cerium, selenium, silicium, zirconium, and colombium, were discovered by him. He likewise proved the metallic nature of ammonium, or the radical composing ingredient of ammonia, as well as the acid properties of silex, and the different degrees in which sulphur combines with platina, phosphorus, &c. He made numerous researches respecting the acid salts of sulphur, hydrofluoric acid, and the fluorides. The new classifications which resulted from some of these discoveries have been of the greatest practical advantage. He felt the necessity of creating new rules for defining all combinations, so as to indicate the properties of each body, which was impossible by the ancient nomenclature, except in relation to the compound oxids. His work on nomenclature commands at once the admiration and gratitude of all who are occupied with chemistry. It may be affirmed, that Berzelius has laid the foundations of organic chemistry. When the atomic theory of Dalton, and the discovery of the alkaline metals by Davy, produced a revolution in science, Berzelius immediately applied the doctrines of the former to the constitution of composite bodies, and to the order of combination of the different elements. Revising all the works of his predecessors, and conducting his experiments with a degree of accuracy hitherto unknown, he determined, by innumerable analyses, the laws which regulate chemical combinations, which he reduced to a degree of simplicity, which rendered them still more admirable. When these laws were once well ascertained, it became possible to control the results of analyses,—even to foresee a great number of combinations then unknown, and to carry into every operation an accuracy previously thought altogether unattainable. Not limiting their application to the composites which might be formed by the chemist, Berzelius soon procured for mineralogy the means of determining, scientifically, a great portion of the substances presented by nature, and which, up to that time, could not be made to enter into any classification of a truly scientific character. He united these two sciences so intimately, that the study of minerals could no longer be separated from chemistry.

The explanation of the theory of chemical proportions will be always regarded as one of the most important services which this chemist has rendered to the science. He published his researches in 1807, before Dalton's ideas were generally known, working according to the almost forgotten views of Richter, which demonstrated the constancy of the combining proportions of acids and bases. The clear judgment of Berzelius enabled him to perceive the value of Richter's notions.

He made very careful analyses of certain salts, and could thus determine the composition of many others. In order to prove the accuracy of Richter's theoretical ideas, he undertook an extensive examination of salts; and when the atomic theory of Dalton subsequently came to his knowledge, he found that it perfectly agreed with the results he had obtained.

He proved, besides, in the most exact manner, that the proportion of oxygen is constant in all the neutral salts of the same acid. Berzelius then determined the relative proportional weights in which the different elements unite in order to form compounds.

This was one of the subjects in which he engaged with the greatest ardour. We are likewise indebted to him for the greater part of the equivalents of simple bodies.

This great chemist not only contributed to establish and bring to perfection the atomic theory, but he introduced it into science, thus giving a powerful impulse to organic and mineralogical chemistry.

The electro-chemical theory, with all its consequences, whether realised or yet to come, is also one of the most remarkable works. This theory has been vigorously assailed in latter times, but up to the present time it has not really been shaken; the application of the laws of combination to the animal and vegetable organisation is, we believe, one effect of the most beautiful results obtained by the power of his genius.

In analytical chemistry Berzelius was indefatigable. From the time that Bergmann gave the first idea of exact analysis, many of the learned have engaged in this important branch of chemistry; but Berzelius' methods excelled all that has been done in accuracy.

We owe to him the best processes for the quantitative separation of different substances; and he determined the composition of a greater number of natural or artificial compounds than any other chemist. Among the most important analytical processes for which science is indebted to him, we may mention the use of hydrofluoric acid in the analysis of siliciferous minerals; also the use of chlore for the separation of metals. His analyses of different minerals, of the mineral waters of Bohemia and other localities, cannot be surpassed in accuracy. Qualitative analysis was likewise greatly improved by his exertions; and the application which he made of the blowpipe has rendered the greatest services to mineralogical researches.

The Swedish chemists, among whom Gahn deserves to be particularly mentioned, have made the most valuable use of the blowpipe as a means of testing minerals. Although at that time scarcely employed in France, this important instrument became, in the hands of Berzelius, one of the most correct means that could be employed in the analysis of inorganic substances. In a work on this instrument, he has pointed out its utility, and the advantages to be derived from the use of it. (*On the Use of the Blowpipe in Chemical Analysis and Mineralogical Determinations.* Translated from the Swedish by F. Fresnel. Paris, 1827.)

It would be impossible, without entering into very minute details, to enumerate even the titles of all Berzelius' Memoirs: few chemists have published so great a number. Scarcely any substance can be mentioned which he did not make the subject of experiment, and each of his investigations comprehends some new method, or some modification of known processes, which may admit of useful application in science.

Berzelius never published a theory which did not rest on facts, and was corroborated by long experience. It is not long since we witnessed numerous discussions on theoretical views; but the illustrious Swede considered a theoretical idea as definitive when it had once been admitted into science, unless it was overthrown by the force of indisputable facts. In chemistry, Berzelius opposed many speculative theories, which he admitted, notwithstanding, to be ingenious; but he gave a preference to older opinions, until new results were found to have a tendency to strengthen them. If some of his own opinions are not adopted by all chemists, this must be ascribed to his excessive caution and circumspection. In a science entirely founded on experiment, this reserve may prevent the admission of a true idea, but, on the other hand, it very seldom leads to error. When he commenced his labours at Upsal, the whole science consisted of a mass of crude theories soldered together, and hasty attempts were made to fill up the most obvious voids by fanciful notions having no resemblance to truth. These were the greatest obstacles he had to overcome; and hence arose the repugnance he always shewed for that mania for theories, which, usurping the place of true philosophy, has built hypothesis on hypothesis, and given the name of science to results which are nothing less than absurd. It would not, however, be quite right to say that Berzelius too much depreciated investigations of a purely theoretical kind; but from this tendency, although carried a little too far, this important advantage arose, that when Berzelius adopted a theory it might be considered as resting on a secure foundation.

This circumspection has often exposed him to severe reproach, and yet it was attended with excellent results for science; for no theoretical idea could be introduced into chemistry with impunity, when there was such an authority to discuss it in all its bearings, and thus test its real value. Without wishing to find fault with the meritorious efforts of those who have endeavoured to introduce new ideas into the science, we nevertheless think that Berzelius has done more by his cautious and analytical spirit, than the greater part of those who have adopted new ideas without previous examination, and who, when they have become general, have boasted loudly of their foresight and perspicacity. It was natural that a man so excessively careful and precise in his own researches, should judge with severity of the labours, and especially the presumed discoveries, of others. Some have presumed to ascribe this tendency in our philosopher, when acting as a critic, to a jealousy unworthy of his noble nature, and have declined to regard it as a proof of the ardent love he bore to the science which occupied every hour of his existence.

Berzelius was jealous only for chemistry. Considering his extensive experience, he could not be otherwise than opposed to the imaginary theories in which the ardent spirit of innovators delights to indulge. If he clung eagerly to old truths, his conduct shews most satisfactorily that such a disposition was no way incompatible with persevering research for what yet remained to be discovered.

Berzelius' investigations on animal chemistry are likewise very important; we may mention particularly, those relating to the blood, bile, and other parts of organism. He discovered the presence of lactic acid in the different animal fluids, such as blood, milk, urine, tears, &c., a discovery which was of great importance for medical science, that is to say, for the chemistry of life.

Electricity, vegetable chemistry, and physiology, have been enriched by the labours of this illustrious chemist. He improved everything he touched, and we may say of him, without fear of contradiction, that he was at once the most indefatigable and most profitable labourer that ever appeared in the field of science.

After having spoken of Berzelius, in relation to his personal works, it remains for us to consider him as a critic. In this respect, he has unquestionably exercised as much influence on the sciences as by his discoveries.

The examination and criticism to which, for upwards of twenty years, Berzelius submitted the works of chemists and and natural philosophers in his *Comptes Rendus annuels,* have often excited against him the anger of authors, who sometimes thought that he spoke of them with too great freedom.

In our day, it has even been said, that few French works found favour in his eyes, unless they were written in the spirit of his doctrines, or modelled after his theories. This is one of the most unjust accusations that could be advanced against him; and we have only to peruse his interesting *Comptes Rendus*, to be convinced that it is altogether false. We might have wished, indeed, that the conscientious work of Berzelius had been a simple statistical view of the progress of the science, instead of being a report on that progress; that is to say, at once a rational exposition, and a judgment, with the reasons on which it was founded.

Others have alleged that these official judgments had no object or utility; that to expose, decide upon, and combat researches which required long and laborious study, was a bad and ungracious undertaking. Some have even gone the length of saying that these reports are not his own work, but merely a compilation made by obliging and inexperienced pupils!

When scientific criticism is conscientiously conducted, we shall always plead its defence against detractors; and those who love science for its own sake, and not for the reputation it sometimes confers, or the profitable employment which it still more rarely procures, will certainly be of our opinion. It is of the highest importance that error should not be introduced into science, and it is consequently necessary that eminent men should be found willing to devote themselves to the toilsome task of examining the discoveries and works which every day appear, in order to sanction with their authority such as are true, to do justice to such as their authors produce, and endeavour in this way to enrich the common domain. It is even one of the greatest misfortunes we have to lament, that the death of Berzelius has left us completely destitute of this scientific criticism; for he alone, if we may so speak, stood as a sentinel in advance surveying the horizon, ever ready to assail rash or false theories, ill-conducted experiments, or factitious explanations.

Far from finding fault with Berzelius' courage in his frank and distinctly-expressed appreciations of the deserts of others, we eagerly hope that others will be found to imitate his glorious example. Criticism has the great advantage of exciting emulation, of drawing the attention of competent judges to experiments and theories, which would spring up only to perish, if they were not resisted. Who does not know, and is not ready to confess, that a serious examination, even though hostile and severe, is much preferable to the subject being allowed to pass over in silence?

But in order to criticise the works of others, and form a judicious and intelligent estimate of them, a union of qualities is required, which unfortunately are rarely met with in a single individual, but which Berzelius possessed in a high degree. We do not indeed pretend that this illustrious chemist was without faults, that he was a diamond without a flaw; Alas! No! he was a man, and as such, liable to error. But we maintain against all, that few learned men have united in the same degree, eminent and indisputable merit, a theoretical and practical superiority universally recognised, a profound knowledge of all that has been done; and, finally, the consciousness that he had a duty to fulfil, a mission to execute.

Yes, we regret Berzelius, for we can never forget the eminent services he rendered every day to science, and we lament to see the threshold of the temple henceforth without a guardian, admitting the entrance of every crude theory, and every vagary of the imagination. Chemistry, in our day, is taking a wrong direction, and the eye of the philosopher observes it with sorrow again entering upon the dark path from which the past century had scarcely extricated it. Chancellor Bacon, the mystic Paracelsus, and before them, our countryman Van Helmont, had however pointed out the superiority of experiments to preconceived theories, and the worthlessness of systems formed before experiment! In the present day, there is no unity in the work; some seek for a new classification, or a new system; they imagine that they find a system while seeking for it, and giving, according to the expression of a modern author (Kirèevsky *Historie des legislateurs chimistes*), *a new aspect to the great work;* they assume altogether the character of the ancient alchemists. Others endeavour to teach organic chemistry; they mystify it,—they do nothing that is of any use. Ought not a new Berzelius to seize the sceptre fallen from the lifeless hands of the illustrious critic, in order to recall these fruitless labourers to order, and shew them by his example, how science should be promoted ? . . . .

In his personal relations Berzelius was simple and plain, without those pretensions which, arising from an exaggerated notion of their own importance, sometimes diminish the pleasure which ought to be derived from the company of men eminent in science. He rose at an early hour, and no visitor ever found him unoccupied. No one, whoever he might be, could ever complain of his reception. He knew the full value of time, and he endeavoured to make others know it also. During a career of seventy years, forty-four of which were passed in the same city, engaged without intermission in difficult, and sometimes painful undertakings, Berzelius knew how to preserve the attachment of his pupils, the friendship of his colleagues, the esteem of his sovereign, and the respect of all. Many of the most distinguished chemists of the age resorted to his laboratory, such as Mitscherlich, Gmelin, Henri, and Gustavus Rose, Wœhler, Magnus, Arfwedson, Mosander, &c. All entertained a boundless respect for their master, for they regarded him as the primary cause of their success in science, as the spirit which formed their minds, and gave a proper direction to their studies.

Perhaps we shall be accused of having attempted an eloge of Berzelius; but our ambition has not been so aspiring. While defending this great man from the unjust reproaches with which he has been assailed, it has been our desire, on this solemn occasion, to call to mind his principal titles to fame. In describing the course of a life as lengthened as it was well employed, we have endeavoured to shew to all young chemists, that present tendencies may lead them astray, and how they may succeed in laying the foundation of an imperishable reputation, and a glorious name ; how, in order to advance the experimental sciences, it is necessary to sustain natural genius by a steadfast perseverance, and a continual labour, which nothing should discourage.

On the Relations of Trap-Rocks with the Ores of Copper and Iron, and the similarity of the Schalstein of Dillenburg, the Blatterstein of the Harz, and the Gabbro of Tuscany.

(Continued from vol. xlvi., p. 306.)

I. *The Relations of Trap-Rocks with Ores of Copper and Iron.*

This extension of geognostic relations between repositories exclusively cupriferous and the trap rocks, gives some importance to a more detailed examination than we have had occasion to make, of the copper ores and traps of Dillenburg. We find, indeed, between the veins of this country characterised by copper pyrites and the greenstones, which form the principal features of the *accidents* and composition of the country, relations different from those we have pointed out in Tuscany, and which add some facts to the relations which, in so many instances, render copper ores subordinate to trap-rocks.

If we examine the environs of Dillenburg on the geological map of Germany, we will perceive that this country, lying towards the northern border of Nassau, forms an islet in the transition mass remarkable for its peculiar composition. Narrow zones, composed of alternations of slates and limestones, supposed to be Devonian, run from the south-west to north-east, following the general direction of the great zone of anthraxiferous limestones, which traverse Belgium and Rhenish Prussia, more to the north. These slates and Devonian limestones lie above the greywackes and Silurian slates of the mass, interrupted by strongly-developed trap-rocks, arranged in zones following the same direction. A second group of the same rocks, affecting the same disposition, is found a little more to the south from Limburg to Weilburg and Braunfels.

II. *Relations of the Schalstein of Dillenburg, the Blatterstein of the Harz, and the Gabro of Tuscany.*

The trap-rocks of Dillenburg cover a considerable surface (8 to 10 square leagues), without, however, being very conspicuous, because their blunted forms present only gentle slopes covered by an active vegetation. But they can be studied in numerous excavations, where we perceive the massive structure of the greenstones, a structure often globular, so as to present mammelated surfaces.

The tissue of these rocks is generally homogeneous and compact; their colours deep, often ochreous on the surface, but greenish in the fractures which reach the unaltered rock. These appearances, moreover, are subject to variations sufficiently indicated by the multitude of names applied to them, such as greenstone, traps, variolites, amphibolites, diorites, &c. If we look for analogous formations, these rocks cannot be better compared than with those, which, in fact, bear the same names in the group of the Harz mountains.

If we study attentively some of the principal trap masses, we observe that the central part is pretty constant in its characters; it is a green rock, homogeneous and compact, the true type of greenstone. The variations which have caused so many different names to be applied to it, occur principally towards the exterior zones; a condition which we have pointed out in the serpentine masses of Tuscany, and which likewise exists in the greenstone of the Harz. If we examine the true rocks of contact, we shall find them exhibiting characters still more complex, but which always remind us of some of those of the trap type.

These rocks of contact are designated at Nassau by the general denomination of *schalstein.*

It is very difficult to define schalstein. It is most frequently a compact and lithoid rock, green or reddish, much rent, especially in the general direction of the stratification; some varieties are even slaty, others are brecciform and massive. The red colour of schalstein sometimes becomes very deep, and it contains, occasionally, conformable beds of red peroxide of iron. Lastly, the variolitic amygdaloids, with calcareous nodules, also form part of the schalstein, and develop themselves more especially in the parts of the locality where the Devonian limestones exist.

The schalstein has long attracted the attention of those who have studied the rocks of Nassau. Becher, Walchner, Stifft, Leonardt, de Dechen, &c., have described the ch ters of these rocks, and distinguished from the schals properly so called, 1*st*, The *kalktrap*, which are com rocks, homogeneous, green or red, characterised by a ture of limestone with the elements of the greenstone; The *mandelstein*, which are nothing else than our amy loids, rocks of contact which connect the preceding with greenstone.

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All these observers appear to agree in regarding the sch stein, kalktrap, and mandelstein, as rocks subordinate greenstone, forming the passage between the crystall rocks and the argillaceous or limestone rocks. Some them, however, have separated the argillaceous schalste which they consider as a normal rock subordinate to the sl rocks; while the kalktrap and the mandelstein cannot supposed to have any other origin than the greensto With regard to ourselves, we consider all these varieties metamorphic rocks.

If we set aside the amygdaloids, the schalstein exhibits t greater part of the characters assigned to the green and r gabbro of Italy. By the amygdaloids and subordinate be of oligistic iron, they become confounded with the blatte stein of the Harz.

All the considerations we have brought forward to she that the gabbro is stratified, may be applied to the scha stein and blatterstein, for these three types of rocks prese remarkable resemblances in the conditions of their pos tions. All the three are found towards the outskirts of th trap-rocks to which they are subordinate, follow the contours of the masses, and, at the same time, the stratified directions of the upraised deposits. All the three exhibit distinct mineralogical transitions, which unite, on the one hand, the rocks evidently eruptive, and, on the other, the rocks evidently stratified. Finally, all the three are partially charged with red peroxide of iron, too abundant to admit of the supposition that it arises from the superoxidisation of the pre-existing iron, and which, according to all probability, ought to be ascribed to special emanations.

On studying the relations of the schalsteins with metalliferous repositories, we shall find further occasion to point out other identities. Let us take an example which will afford us an opportunity, in the first place, of stating precisely the conditions of the position (*gisement*) of the greenstones, the schalsteins and stratified rocks of Dillenburg, and then to describe the direction and composition of the metalliferous repositories found in these formations.

To the north of Dillenburg, the valley of the Dill is enclosed by mountainous groups which contain mines of copper worked for a long period, but now only on a small scale. On the left bank, an English company has opened pretty extensive works in the neighbourhood of Nanzenbach; and, on the right bank, many German companies are scattered about, particularly above the village of Weidmansheil, where the mines of Stangenwaage, Bermausglücke, Gnade-Gottes, and Haus-Nassau are situated. The formation containing these varied repositories is in very strongly inclined strata, running north-east, south-west, following the general course of the devonian and silurian strata of the country; so that the projection of the different constituent strata forms a succession of rather unequal parallel bands which intersect the valley of the Dill. These beds belong to the bluish clay-slate, alternating with some calcareous beds, and form the principal mass of the formation; but they are interrupted by large masses of greenstone, which are most frequently inserted conformably to the planes of stratification, and shew themselves with all their rocks annexed. These annexed rocks are the red clay-slate, which represents the first degree of alteration in the upraised formation and the s stein.*

One may study the different rocks of the formatio going to the mines by the narrow road to the north-wes Dillenburg, which runs along the side of the naked esca ments. We can distinguish the massive and undulated faces of the greenstone at a great distance, and, in the heaps, the most varied specimens are to be found of the r traversed by the mine galleries, which are mostly pie perpendicularly to the direction of beds, so as to cut ac the whole of them. By the aid of plans of the mines, wh the different formations are marked in accordance with description of the metalliferous repositories, we gain a pre knowledge of their mineralogical characters, and rela position.

In a mineralogical point of view, the schalstein, wh characters are so variable, are the most interesting roc We find among them brecciform varieties, with green or r dish angular fragments; amygdaloidal varieties, with a m ture of calcareous spar, which is sometimes in irregular vei sometimes in radiated globules; lastly, we find among th those green or red varieties, compact and homogeneo which throw so much uncertainty over the origin of the rocks. Among the rocks connected with the schalstein, a having the same direction, we may mention the red peroxi of iron, which forms irregular beds from 1 to 3 or 4 yar in thickness. At the mine of Stangenwaage there is a thi bed of this oligistic iron found on the roof of a bed of scha stein, and which exhibits all the peculiarities of position o served in banks of oligistic iron subordinate to the blatterste of the valley of Lehrbach, in the Harz. In the mining hea we are struck with the flat shape of all the fragments schalstein, and on examining the rock *in situ*, we perceiv that the principal fissures which produce this structure, an

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[*] There is still a further approximation to be noticed between the altere rocks of Dillenburg and those of Tuscany. The two degrees of alteration ar represented in the two localities; the first by the red slates of Nassau and th red galestri of Tuscany; the second by the schalstein and the gabbro.

even sometimes give the rock a slaty structure, are parallel to the planes of stratification.

With respect to the geological situation of the schalstein, we may mention a fact observed in the mine of Stangenwaage; namely, that the beds of schalstein are sometimes contained between the slaty or calcareous beds, without being in immediate contact with the greenstones. This isolation is pretty frequent, and, if we connect it with this other fact, that the greenstone is still more frequently in immediate contact with the slaty formation without intermediate borders of schalstein, we come to the conclusion, that the origin of the schalstein is not simply owing to circumstances of contact. These rocks, as well as the beds of oligistic iron, to which we shall afterwards advert, probably arise from complex and prolonged phenomena of emanations which have followed the eruptions of the trap.

Let us now examine the conditions of the cupriferous repositories. These repositories consist of pretty numerous veins; some of them, continuous and rather wide, follow a general direction, perpendicular to that of the beds, although it is somewhat tortuous. These are the master veins. Others, much more numerous, are short and very narrow; their direction is generally oblique, and they are often confounded with the planes of stratification.

The principal vein of Stangenwaage (*haupt-gang*) traverses in this way the series of all the beds of the formation, and is consequently found in very heterogeneous media. The principal contents are quartz, to which may be added, in greater or less quantity, peroxide of iron, and the debris coming from the rocks of the roof and walls. Copper pyrites, pure, and often crystallized, are found in these vein-stones (*gangues,*) and the experience of the miners has long since proved, *that the veins are never wide and rich in copper pyrites, but when they traverse the greenstone and schalstein.*

The works of the mine of Stangenwaage demonstrate the truth of this law. The miners have given the name of enrichment (*edle-mittel*) to the parts which contain the copper pyrites in largest proportion; now these enrichments exist only when the veins cross the above-mentioned rocks.

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To the north of Dillenburg, the valley of the Dill is enclosed by mountainous groups which contain mines of copper worked for a long period, but now only on a small scale. On the left bank, an English company has opened pretty extensive works in the neighbourhood of Nanzenbach; and, on the right bank, many German companies are scattered about, particularly above the village of Weidmansheil, where the mines of Stangenwaage, Bermausglücke, Gnade-Gottes, and Haus-Nassau are situated. The formation containing these varied repositories is in very strongly inclined strata, running north-east, south-west, following the general course of the devonian and silurian strata of the country; so that the projection of the different constituent strata forms a succession of rather unequal parallel bands which intersect the valley of the Dill. These beds belong to the bluish clay-slate, alternating with some calcareous beds, and form the principal mass of the formation; but they are interrupted by large masses of greenstone, which are most frequently inserted conformably to the planes of stratification, and shew themselves with all their rocks annexed. These annexed rocks are the red clay-slate, which represents the first degree of alteration in the upraised formation and the schalstein.*

One may study the different rocks of the formation by going to the mines by the narrow road to the north-west of Dillenburg, which runs along the side of the naked escarpements. We can distinguish the massive and undulated surfaces of the greenstone at a great distance, and, in the mine heaps, the most varied specimens are to be found of the rocks traversed by the mine galleries, which are mostly pierced perpendicularly to the direction of beds, so as to cut across the whole of them. By the aid of plans of the mines, where the different formations are marked in accordance with the description of the metalliferous repositories, we gain a precise knowledge of their mineralogical characters, and relative position.

In a mineralogical point of view, the schalstein, whose characters are so variable, are the most interesting rocks. We find among them brecciform varieties, with green or reddish angular fragments; amygdaloidal varieties, with a mixture of calcareous spar, which is sometimes in irregular veins, sometimes in radiated globules; lastly, we find among them those green or red varieties, compact and homogeneous, which throw so much uncertainty over the origin of these rocks. Among the rocks connected with the schalstein, and having the same direction, we may mention the red peroxide of iron, which forms irregular beds from 1 to 3 or 4 yards in thickness. At the mine of Stangenwaage there is a thick bed of this oligistic iron found on the roof of a bed of schalstein, and which exhibits all the peculiarities of position observed in banks of oligistic iron subordinate to the blatterstein of the valley of Lehrbach, in the Harz. In the mining heaps we are struck with the flat shape of all the fragments of schalstein, and on examining the rock *in situ*, we perceive that the principal fissures which produce this structure, and even sometimes give the rock a slaty structure, are parallel to the planes of stratification.

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* There is still a further approximation to be noticed between the altered rocks of Dillenburg and those of Tuscany. The two degrees of alteration are represented in the two localities; the first by the red slates of Nassau and the red galestri of Tuscany; the second by the schalstein and the gabbro.

With respect to the geological situation of the schalstein, we may mention a fact observed in the mine of Stangenwaage; namely, that the beds of schalstein are sometimes contained between the slaty or calcareous beds, without being in immediate contact with the greenstones. This isolation is pretty frequent, and, if we connect it with this other fact, that the greenstone is still more frequently in immediate contact with the slaty formation without intermediate borders of schalstein, we come to the conclusion, that the origin of the schalstein is not simply owing to circumstances of contact. These rocks, as well as the beds of oligistic iron, to which we shall afterwards advert, probably arise from complex and prolonged phenomena of emanations which have followed the eruptions of the trap.

Let us now examine the conditions of the cupriferous repositories. These repositories consist of pretty numerous veins; some of them, continuous and rather wide, follow a general direction, perpendicular to that of the beds, although it is somewhat tortuous. These are the master veins. Others, much more numerous, are short and very narrow; their direction is generally oblique, and they are often confounded with the planes of stratification.

The principal vein of Stangenwaage (*haupt-gang*) traverses in this way the series of all the beds of the formation, and is consequently found in very heterogeneous media. The principal contents are quartz, to which may be added, in greater or less quantity, peroxide of iron, and the debris coming from the rocks of the roof and walls. Copper pyrites, pure, and often crystallized, are found in these vein-stones (*gangues,*) and the experience of the miners has long since proved, *that the veins are never wide and rich in copper pyrites, but when they traverse the greenstone and schalstein.*

The works of the mine of Stangenwaage demonstrate the truth of this law. The miners have given the name of enrichment (*edle-mittel*) to the parts which contain the copper pyrites in largest proportion; now these enrichments exist only when the veins cross the above-mentioned rocks.

We may perceive that there is here a law determining the richness of veins, which may be explained by this fact observed in the veins of other countries, namely, that the fissures are ill developed in slaty formations, so that the veins in them are narrow, and filled with steril debris; while the greenstone and schalstein, by the open and distinct nature of their fractures, have presented wider and more durable inlets to the metalliferous emanations. A second law, however, comes to add to the importance of the first, and authorizes us to attribute a more direct and decided metalliferous influence to these rocks; namely, that these vein-fissures, which from their origin have but little dependence on the enclosing rocks, exist only in positions analogous to those we have described, positions really subordinate to the trap-rocks.

Accordingly, in the vast transition mass of the Rhenish Provinces, the characteristics of mineral riches are sparry iron, blende, and galena; few copper mines, properly so called, exist there, unless it be some veins, forming an exception to the general arrangement at Rheinbreitenbach. Throughout the whole trap country of Dillenburg, we observe, on the contrary, a multitude of veins, exclusively characterized by oligistic iron and copper pyrites, and the richest places there are always in the relations of vicinity or contact with the trap-rock. When we leave the traps, for example, and repair to the county of Siegen, so rich in ores, we find that the copper pyrites becomes only an accidental mineral.*

The constitution of the metalliferous formation of Dillenburg, and the relations which regulate the richness of the veins, give rise to a very interesting arrangement.

The alternating beds which form the mass of Stangenwaage, are highly inclined, and dip at angles from 55 to 75 degrees. Now, as the veins which cross these alternations almost perpendicularly to their direction, become rich in the greenstones and schalstein which dip under the same angles, it follows that the metalliferous zones of the veins are inclined as the vertical sections of these beds. Thus, then, by referring the *allure* of the metalliferous zones to the direction and inclination of the veins, we find *that the zones of enrichment follow inclined lines, diagonal between the inclination and direction of the veins.*

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* I am indebted to M. Heusler, chief mining-engineer in the district of Siegen, for the communication of these results, attested by long practice in the mines of the whole of that country.

This diagonal allure of the metalliferous zones in veins, is not an exceptional fact; examples of it are mentioned in the veins which occur on the right bank of the Rhine, from Holzappel to St Goar; but here this fact is explained by the influence and direction of the enclosing rocks. This, then, is still another example of the study of the theory of veins and their geognostic relations serving as a guide to miners. We see, in fact, that vertical works, undertaken to intersect at some depth the ascertained profitable parts in the first levels, might lead to steril zones; we might be misled by want of success in this way, and be induced to declare that the veins presented no guarantee of richness in depth.

The subterranean origin of ores in the metalliferous repositories is no longer, in the present day, considered doubtful; but among the facts which demonstrate this origin, a first place must be assigned to their almost constant connection with the eruptive rocks. All the proofs furnished by the characters of volcanoes, or by certain igneous masses which contain ores, such as the amphibolites of Tuscany, the traps of Kewena Point, the greenstones of Siberia, the serpentines of Reichenstein in Silesia, &c., may in strictness be rejected, as resulting from local and limited facts, an objection which cannot be made to facts so vast and general as geognostic relations. These relations change their form, they are more or less direct, but when we see them reproduced at the most remote points of the globe, and over vast surfaces—when we find them inscribed on the plans of mines, and in the language of workmen, we cannot fail to consider them as furnishing a most convincing argument in favour of the subterranean origin of ores.

The greenstones of the neighbourhood of Dillenburg afford a remarkable case of the dissemination of ores in the very paste of the eruptive rock. It is a dyke, from 5 to 10 yards broad, penetrated with sulphuret of nickel in crystals or needles, which penetrate the whole paste in such a way as to leave little doubt as to the fact of their being contemporary. The mining, which is of old date, has found a source of the production of nickel in these greenstones of great interest, for it is employed on an ore hitherto very rare, and whose eruptive origin cannot be doubted.

The connection of the Dillenburg schalstein with the ores of copper, is a consideration to be added to those on which we have rested their assimilation to the gabbro of the North West of Italy; on the other hand, their still more intimate connection with the repositories of oligistic iron, diffused abundantly throughout Nassau, identifies them in a still more direct manner with the blatterstein of the Harz.

We have pointed out this law ("Etudes sur les mines") which regulates the positions of the oligistic iron of the Harz, especially in the valleys of Lerbach and Altenau, namely, that these ores belong to repositories of contact subordinate to the blatterstein and greenstone, and even inserted accidentally into their own mass. This law is expressed in the mine of Stangenwaage, where we see a thick repository of oligistic iron forming a *salbande* to the schalstein, and other small beds of less importance entering into the very mass of the schalstein and greenstone, always parallel to the general plane of stratification.

The repositories of peroxide of iron are still more numerous in Dillenburg than in the Harz, and always in the same conditions as to position. In order to give an idea of their abundance, we may mention the fact that the foundries in the neighbourhood of Herborn may derive their ores from forty repositories either mined or known. This richness in iron-ore extends to the group of greenstone south from Nassau. There are likewise banks of oligistic iron, of all dimensions, subordinate to the schalstein, so that a certain number of them has been mined to levels accessible by drainings not of an expensive character, while others have furnished ores from time immemorial. These ores, which are rich and of good quality, are sold for not more than seven francs a ton.

Recapitulating what has been stated respecting the relations of greenstone and schalstein with metalliferous repositories, we find, then, the trap-rocks, 1*st*, Exerting enriching influences on numerous cupriferous veins, whose development is also subordinate to them ; 2*d*, Containing accidentally oligistic iron enclosed in globular shapes in their masses, and sulphuret of nickel disseminated in contemporaneous crystals; 3*d*, Presenting relations of contact with the multiplied repositories of oligistic iron.

The schalstein of Dillenburg, the blatterstein of the Harz, and the gabbro of Italy, rocks which we have assimilated, as resulting, the whole three, from metamorphic influences developed at the point of contact with the masses of trap, have a common character of the most striking kind, which is the strong red colour they impart to a great portion of the rocks they contain. In their normal state, these rocks are green, and exhibit, in a more or less distinct form, the characters of the trap masses to which they happen to be subordinate, establishing the passage between the eruptive rock and the upraised stratified rocks. The proportion of protoxide of iron which they contain in this normal state, prevents us supposing the reddening here to be the result of a simple superoxidation of pre-existing iron; the iron is superadded, and in such quantity that the rocks are connected by transitions and relations of contact to concentrations of pure ores.

In order to account for the reddening of these metamorphic rocks, we have, therefore, strong reasons to admit the same theoretic explanation as for the generation of the subordinate ores ; and this explanation necessarily extends to the simple reddened clay-slate of Dillenburg, the red flinty slate of the Harz, the galestri of Tuscany, and those red jaspers which the Italian peasants so expressively name *mattoni* (bricks). Now, in the present state of our geological knowledge, we can only ascribe this generation of oligistic iron to subterranean emanations ; these emanations have followed the outbursting of the trap-rocks, since we find the products of them in certain veins which intersect the traps ; they are, therefore, to the trap rocks, what the products of *Solfataras* now are to the volcanoes of the present period.

Concentrations of oligistic iron in highly crystalline repositories such as that of Rio in the island of Elba, leave no doubt upon the mind. We can conceive the posterior arrival of these ores, under a form sufficiently subtile to penetrate into all the fissures of a formation, to saturate all the mineral mass, and become insulated in wide veins. This Rio repository bears, indeed, all the characters of a slow generation, by the prolonged action of vapours analogous to those which bring the oligistic iron into the craters of volcanoes. The lustre, the geodes incrusted with crystals, the perfect isolation of the crystals of pyrites which formed special groups, and the corrosion of these pyrites, which are often transformed into oligistic iron; all these details seem to combine in indicating the prolonged action of metalliferous vapours. We see that, in many cases, the oligistic iron; when in the micaceous form which, under the hammer, affords a light and brilliant powder, is posterior to the oligistic iron, which is compact or in binoternary crystals. Is not this same subterranean action further evident in the semi-crystalline repository of Framont, which has produced the repositories of the Harz and Nassau, which differ from it only in their less crystalline nature? Ought not the lithoid oligistic iron, which impregnates the schalstein, blatterstein, and gabbro, be ascribed to the same causes, which are here marked by the same conditions of position, and finally the red hue of the stratified rocks, such as the red clay-slate, the gallestri, mattoni, &c.

By generalising this theory, we shall be led to even more extended conclusions. In certain sedimentary formations, we find earthy oligistic irons concentrated or disseminated in the red-coloured rocks. Formations of the old and new red sandstone, the sandstone of the Vosges, the varied coloured marls, and generally the gypseous and saliferous marls of the secondary or tertiary formations, present us with numerous and well-developed examples, either of the general or partial coloration of deposits by oligistic iron. Among these deposits we find beds of concentrated ores, compact or oolitic (Lavoulte, Laverpillère, Privas, &c.,) and in these beds shells, themselves transformed into compact or even crystalline ores.

What are the phenomena which could have accumulated in particular beds, or disseminated, through entire formations, such considerable quantities of peroxide of anhydrous iron; while we cannot well conceive the iron deposited from waters, in any other state than that of hydrated peroxide? When we examine the immense quantity of oligistic iron disseminated through red-coloured arenaceous formations, we can form only two hypotheses; either this mass of peroxide has been derived, like the other arenaceous elements, from the pre-existing rocks; or, it has been superadded, by means of special phenomena, in those same basons where the sedimentation took place. The first of these suppositions is scarcely admissible; and we are led, by every thing that has been previously said, to have recourse to the phenomena of subterranean emanations, contemporary with the deposits, and mingling their products with those of the sedimentation.

In support of this hypothesis, we may mention the remark made by M. Elie de Beaumont, that the presence of stratified dolomite, gypsum, anhydrite, and rock-salt, almost always concurs with the red colour of the deposits. Now, all have nearly agreed in regarding all these substances as originating in metamorphic actions contemporaneous with the deposits in which they are formed.

Thus, throughout the whole duration of geological times, the interior of the globe should appear to us as a centre of continuous emanations, which have sent enormous masses of iron to the surface; these emanations mingling their anhydrous products, sometimes with those of sedimentation, at other times interposing themselves under the form of concentrated repositories, in the rocks elevated by the eruptive masses.—*Amédée Burat.*[*]

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[*] From *Annales des Mines*, t. xiii., p. 351–378.

On an Equation between the Temperature and the Maximum Elasticity of Steam and other Vapours. By WILLIAM JOHN MACQUORN RANKINE, Civil Engineer. (With a Plate.) Communicated by the Author.

In the course of a series of investigations founded on a peculiar hypothesis respecting the molecular constitution of matter, I have obtained, among other results, an equation giving a very close approximation to the maximum elasticity of vapour in contact with its liquid at all temperatures that usually occur.

As this equation is easy and expeditious in calculation, gives accurate numerical results, and is likely to be practically useful, I proceed at once to make it known, without waiting until I have reduced the theoretical researches, of which it is a consequence, to a form fit for publication.

The equation is as follows:—

$$\text{(1.)} \qquad \text{Log. } P = \alpha - \frac{\beta}{t} - \frac{\gamma}{t^2}$$

Where P represents the maximum pressure of a vapour in contact with its liquid:—

t, the temperature, measured on the air-thermometer, from a point which may be called the ABSOLUTE ZERO, and which is—

274°·6 of the *centigrade scale* below the freezing point of water.

462°·28 of *Fahrenheit's scale* below the *ordinary* zero of that scale, supposing the boiling point to have been adjusted under a pressure of 29·922 inches of mercury, so that 180° of Fahrenheit may be exactly equal to 100 centigrade degrees.

461°·93 below the ordinary zero of Fahrenheit's scale, when the boiling point has been adjusted under a pressure of 30 inches of mercury, 180° of Fahrenheit being then equal to 100°·0735 of the centigrade scale.

The *form* of the equation has been given by theory; but three constants, represented by α, β, and γ, have to be determined for each fluid by experiment.

The inverse formula, for finding the temperature from the pressure, is of course

$$(2.)\qquad \frac{1}{t} = \sqrt{\frac{\alpha - \text{Log. P}}{\gamma} + \frac{\beta^2}{4\gamma^2}} - \frac{\beta}{2\gamma}$$

It is obvious that for the determination of the three constants, it is sufficient to know accurately the pressures corresponding to three temperatures; and that the calculation will be facilitated if the reciprocals of those temperatures, as measured from the absolute zero, are in arithmetical progression.

In order to calculate the values of the three constants, for the vapour of water, the following data have been taken from M. Regnault's experiments :—

Temperatures in Centigrade Degrees.		Common Logarithms of the Pressure in Millimètres of Mercury.	REMARKS.
Above the Freezing Point.	Above the Absolute Zero.		
220°·	494°·6	4·2403	Measured by M. Regnault on his curve, representing the mean results of his experiments.
100·	374·6	2·8808136	Logarithm of 760 millimètres.
26·86	301·46	1·4198	Calculated by interpolation from M. Regnault's general table.

These data give the following results for the vapour of water, the pressures being expressed in millimètres of mercury, and the temperatures in centigrade degrees of the air-thermometer :—

$$\text{Log. } \gamma = 5{\cdot}0827176 \qquad \text{Log. } \beta = 3{\cdot}1851091$$
$$\alpha = 7{\cdot}831247.$$

Table I. exhibits a comparison between the results of the formula and those of M. Regnault's experiments, for every tenth degree of the centigrade air-thermometer, from 30° below the freezing point to 230° above it, being within one or two degrees of the whole range of the experiments.

M. Regnault's values are given, as measured by himself, on the curves representing the mean results of his experiments, with the exception of the pressures at 26°·86, one of the data already mentioned, and that at − 30°, which I have calculated by interpolation from his Table, series *h*.

Each of the three data used in determining the constants is marked with an asterisk*.

In the columns of differences between the results of the formula and those of experiment, the sign + indicates that the former exceed the latter, and the sign − the reverse.

Beside each such column of differences is placed a column of the corresponding differences of temperature, which would result in calculating the temperature from the pressure by the inverse formula. These are found by multiplying each number in the preceding columns by $-\frac{dt}{d\text{P}}$, or by $\frac{-dt}{d\,\text{Log. P}}$ as the case may require.

Air Thermometer from		according to		lation and Experiment in Millimètres.	Differences of Temperature.	according to		tion and Experiment in Logarithms.	Differences of Temperature.	above the Freezing Point.
The Freezing Point.	The Absolute Zero.	The Formula.	M. Regnault's Experiments.			The Formula.	M. Regnault's Experiments.			
° −30	° 244·6	0·35	0·34	+0·01	° −0·42					
−20	254·6	0·89	0·91	−0·02	+0·25					
−10	264·6	2·07	2·08	−0·01	+0·06					
0	274·6	4·47	4·60	−0·13	+0·38					
+10	284·6	9·05	9·16	−0·11	+0·18					
20	294·6	17·33	17·39	−0·06	+0·06					
*26·86	301·46	26·29	26·29	0·00	0·00					
30	304·6	31·57	31·55	+0·02	−0·01					
40	314·6	55·05	54·91	+0·14	−0·05					
50	324·6	92·26	91·98	+0·28	−0·06					
60	334·6	149·15	148·79	+0·36	−0·05					
70	344·6	233·48	233·09	+0·39	−0·04					
80	354·6	355·04	354·64	+0·40	−0·03					
90	364·6	525·70	525·45	+0·25	−0·01					
*100	374·6	760·00	760·00	0·00	0·00	2·8808136	2·8808	0·0000	° 0·00	° 100
110	384·6	1074·82	1073·70	+1·12	−0·03	3·031362	3·0307	+0·0007	−0·05	110
120	394·6	1490·1	1489·0	+1·1	−0·02	3·173204	3·1734	−0·0002	+0·01	120
130	404·6	2028·0	2029·0	−1·0	+0·02	3·307061	3·3076	−0·0005	+0·04	130
140	414·6	2713·8	2713·0	+0·8	−0·02	3·433576	3·4332	+0·0004	−0·03	140
150	424·6	3575·5	3572·0	+3·5	−0·04	3·553334	3·5537	−0·0004	+0·03	150
160	434·6	4643·6	4647·0	−3·4	+0·03	3·666853	3·6676	−0·0007	+0·06	160
170	444·6	5951·2	5960·0	−8·8	+0·06	3·774603	3·7750	−0·0004	+0·04	170
180	454·6	7533·7	7545·0	−11·3	+0·07	3·877005	3·8772	−0·0002	+0·02	180
190	464·6	9428·5	9428·0	+ 0·5	−0·00	3·974443	3·9743	+0·0001		

In comparing the results of the formula with those of experiment, as exhibited in Table I., the following circumstances are to be taken into consideration:—

First, That the uncertainty of barometric observations amounts in general to at least one-tenth of a millimètre.

Secondly, That the uncertainty of thermometric observations is from one-twentieth to one-tenth of a degree, under ordinary circumstances, and at high temperatures amounts to more.

Thirdly, That, in experiments of the kind referred to in the Table, those two sorts of uncertainty are combined.

The fifth column of the Table shews that, from 30° below the freezing point to 20° above it, where the minuteness of the pressures makes the barometric errors of most importance, the greatest difference between experiment and calculation is $\frac{13}{100}$ of a millimètre, or $\frac{1}{200}$ of an inch of mercury, a very small quantity in itself, although, from the slowness with which the pressure varies at low temperatures, the corresponding difference of temperature amounts to $\frac{38}{100}$ of a degree.

The sixth and tenth columns shew that, from 20° to 230° above the freezing point, the greatest of the discrepancies between experiment and observation corresponds to a difference of temperature of only $\frac{8}{100}$ of a degree, and that very few of those discrepancies exceed the amount corresponding to $\frac{1}{20}$ of a degree.

A comparison between the sixth and tenth columns shews that, for four of the temperatures given, viz., 120°, 150°, 200°, and 210°, the pressures deduced from M. Regnault's curve of actual elasticities, and from his logarithmic curve respectively, differ from the pressures given by the formula in opposite directions.

If the curves represented by the formula were laid down on M. Regnault's diagram, they would be almost undistinguishable from those which he has himself drawn, except near the freezing point, where the scale of pressures is very large, the heights of the mercurial column being magnified eightfold on the plate. In the case of the curves of logarithms of pressures above one atmosphere, the coincidence would be almost perfect.

The formula may therefore be considered as accurately representing the results of all M. Regnault's experiments throughout a range of temperatures from 30° of the centigrade scale below the freezing point to 230° above it, and of pressures from $\frac{1}{2200}$ of an atmosphere up to 28 atmospheres.

It will be observed that equation (1.), bears some resemblance to the formula proposed by Professor Roche in 1828, viz.:

$$\text{Log } P = A - \frac{B}{T+C}$$

where T represents the temperature measured from the ordinary zero point, and A, B, and C, constants, which have to be determined from three experimental data. It has been shewn, however, by M. Regnault, as well as by others, that though this formula agrees very nearly with observation throughout a limited range of temperature, it errs widely when the range is extensive. I have been unable to find Professor Roche's memoir, and I do not know the reasoning from which he has deduced his formula.

The use in computation of the equations I have given, whether to calculate the pressure from the temperature, or the temperature from the pressure, is rapid and easy. In Table II. they are recapitulated, and the values of the constants for different measures of pressure and temperature are stated.

In calculating the values of α, the specific gravity of mercury has been taken as 13·596.

Temperatures measured by mercurial thermometers are in all cases to be reduced to the corresponding temperatures on the air-thermometer, which may be done by means of the table given by M. Regnault in his memoir on that subject.

TABLE II. *Vapour of Water.*

Formula for calculating the Maximum Elasticity of Steam (P), from the Temperature on the Air-Thermometer, measured from the Absolute Zero (t):

$$\text{Log } P = \alpha - \frac{\beta}{t} - \frac{\gamma}{t^2}$$

Inverse Formula for calculating the Temperature from the Maximum Elasticity of Steam :

$$\frac{1}{t} = \sqrt{\frac{\alpha - \text{Log P.}}{\gamma} + \frac{\beta^2}{4\gamma^2}} - \frac{\beta}{2\gamma}$$

Values of the Constants depending on the Thermometric Scale.

For the centigrade scale :—

Absolute zero 274°·6 below the freezing point of water.

$\text{Log } \beta = 3{\cdot}1851091$ $\text{Log } \gamma = 5{\cdot}0827176$

$\frac{\beta}{2\gamma} = 0{\cdot}0063294$ $\frac{\beta^2}{4\gamma^2} = 0{\cdot}00004006$

For Fahrenheit's scale ; boiling point adjusted at 29·922 inches.

Absolute zero 462°·28 below ordinary zero.

$\text{Log } \beta = 3{\cdot}4403816$ $\text{Log } \gamma = 5{\cdot}5932626$

$\frac{\beta}{2\gamma} = 0{\cdot}0035163$ $\frac{\beta^2}{4\gamma^2} = 0{\cdot}000012364$

For Fahrenheit's scale ; boiling point adjusted at 30 inches.

Absolute zero 461°·93 below ordinary zero.

$\text{Log } \beta = 3{\cdot}4400625$ $\text{Log } \gamma = 5{\cdot}5926244$

$\frac{\beta}{2\gamma} = 0{\cdot}0035189$ $\frac{\beta^2}{4\gamma^2} = 0{\cdot}000012383$

Values of the Constant α, depending on the Measure of Elasticity.

For millimètres of mercury	$\alpha = 7{\cdot}831247$
English inches of mercury	6·426421
Atmospheres of 760 mil. = 29·922 inches = 14·7 lbs. on the square inch = 1·0333 kil. on the centimètre ² .	4·950433
Atmospheres of 30 inches = 761 mil. ·99 = 14·74 lbs. on the square inch = 1·036 kil. on the centimètre ² .	4·949300
Kilogrammes on the square centimètre .	4·964658
Pounds Avoirdupois on the square inch .	6·117817

N.B.—All the Constants are for *common logarithms.*

I have applied similar formulæ to the vapours of *alcohol* and *ether*, making use of the experiments of Dr Ure.

In order to calculate the constants, the following experimental data have been taken, assuming that, on Dr Ure's thermometers, 180° were equal to 100 centigrade degrees.

	Temperatures on Fahrenheit's Scale from		Pressures in Inches of Mercury.	Remarks.
	the ordinary zero.	the absolute zero.		
	°	°		
For Alcohol, of the specific gravity 0·813.	250	712·3	132·30	From Dr Ure's Table.
	173	635·3	30·00	Do.
	111·02	573·32	6·30	Interpolated in the same Table.
For Ether, boiling at 105° F., under 30 inches of pressure.	200	662·3	142·8	From Dr Ure's Table.
	148·8	611·1	66·24	Interpolated.
	105	567·3	30·00	From the Table.
For Ether, boiling at 104° F., under 30 inches.	104	566·3	30·00	From Dr Ure's Table.
	66·7	529·0	13·76	Interpolated.
	34	496·3	6·20	From the Table.

The values of the constants in equation (1.), calculated from these data, are as follows, for inches of mercury and Fahrenheit's scale:—

	α	Log β.	Log γ.
Alcohol, specific gravity, 0·813.	6·16620	3·3165220	5·7602709
Ether, boiling point, 105° F.	5·33590	3·2084573	5·5119893
Ether, boiling point, 104° F.	5·44580	3·2571312	5·3962460

Absolute zero 462°·3 below ordinary zero.

The curves represented by the formulæ for those three fluids are laid down on the diagram which accompanies this memoir (Plate I.), and which, in the engraving, has been reduced to one-fourth of the original scale. The horizontal divisions represent the scale of Fahrenheit's thermometer, numbered from the ordinary zero;—the vertical divisions, pressures of vapour, according to the scales specified on the respective curves. The points corresponding to the experimental data are surrounded by small circles.

The curve for alcohol extends from 32° to 264° of Fahrenheit. It is divided into two portions, having different vertical scales, suitable to high and low pressures respectively.

The curve for the less volatile ether extends from 105° to 210°; that for the more volatile ether, from 34° to 104°.

The results of Dr Ure's experiments are marked by small crosses.

The irregular and sinuous manner in which those crosses are distributed, indicates that the errors of observation, especially at high temperatures, must have been considerable. This does not appear surprising, when we recollect how many causes of uncertainty affect all the measurements required in such experiments, especially the thermometric observations, and how little those causes have been understood until very recently. The data from which the constants have been calculated, are, of course, affected by the general uncertainty of the experiments.

When those circumstances are taken into account, it is obvious, from inspection of the diagram, that the curves representing the formulæ agree with the points representing the experiments, as nearly as the irregularity of the latter and the uncertainty of the data permit; and that there is good reason to anticipate, that, when experiments shall have been made on the vapours of alcohol and ether with a degree of precision equal to that attained by M. Regnault in the case of the vapour of water, the equation will be found to give the elasticities of those two vapours as accurately as it does that of steam.

Although the diagram affords the best means of judging of the agreement between calculation and experiment, three Tables (III., IV., and V.) are annexed, in order to shew the numerical amount of the discrepancies at certain temperatures. The data, as before, are marked with asterisks.

It is worthy of remark, in the case of alcohol, that although the lowest of the experimental data is at the temperature of 111°·02, the formula agrees extremely well with the experiments throughout the entire range of 79 degrees *below* that point.

Table III.—*Vapour of Alcohol, of the Specific Gravity* 0·813.

Temperature in Degrees of Fahrenheit from the ordinary Zero.	Pressures in Inches of Mercury according to		Differences between Calculation and Experiment in Inches.	Corresponding Differences of Temperature.
	The Formula.	Dr Ure's Experiments.		
32°	0·41	0·40	+0·01	−0°·5
40	0·57	0·56	+0·01	−0·4
50	0·84	0·86	−0·02	+0·7
60	1·22	1·23	−0·01	+0·2
70	1·74	1·76	−0·02	+0·3
80	2·43	2·45	−0·02	+0·2
90	3·36	3·40	−0·04	+0·4
100	4·56	4·50	+0·06	−0·5
110	6·12	6·00	+0·12	−0·7
*111·02	6·30	6·30	0·00	0·0
120	8·10	8·10	0·00	0·0
130	10·61	10·60	+0·01	−0·0
140	13·73	13·90	−0·17	+0·5
150	17·60	18·00	−0·40	+0·9
160	22·32	22·60	−0·28	+0·5
170	28·06	28·30	−0·24	+0·4
*173	30·00	30·00	0·00	0·0
180	34·96	34·73	+0·23	−0·3
190	43·21	43·20	+0·01	−0·0
200	52·96	53·00	−0·04	+0·0
210	64·47	65·00	−0·53	+0·5
220	77·92	78·50	−0·58	+0·4
230	93·54	94·10	−0·56	+0·4
240	111·58	111·24	+0·34	−0·2
*250	132·30	132·30	0·00	0·0
260	155·98	155·20	+0·78	−0·3
264	165·58	166·10	−0·52	+0·2
(1.)	(2.)	(3.)	(4.)	(5.)

Table IV.—*Vapour of Ether—Boiling Point* 105° F.

Temperature in Degrees of Fahrenheit above the ordinary zero.	Pressures in Inches of Mercury according to		Differences between Calculation and Experiment in Inches of Mercury.	Corresponding Differences of Temperature.
	The Formula.	Dr Ure's Experiments.		
*105°	30·00	30·00	0·00	0°·0
110	33.06	32·54	+0·54	−0·9
120	39·98	39·47	+0·51	−0·7
125	43·83	43·24	+0·59	−0·8
130	47·95	47·14	+0·81	−1·0
140	57·10	56·90	+0·20	−0·2
*148·8	66·24	66·24	0·00	0·0
150	67·53	67·60	−0·07	+0·1
160	79·35	80·30	−0·95	+0·9
170	92·68	92·80	−0·12	+0·1
175	99·94	99·10	+0·84	−0·6
180	107·62	108·30	−0·68	+0·4
190	124·29	124·60	−0·51	+0·3
*200	142·80	142·80	0·00	0·0
205	152·78	151·30	+1·48	−0·7
210	163·27	166·00	−2·73	+1·1

Table V.—*Vapour of Ether—Boiling Point* 104° F.

Temperatures in Degrees of Fahrenheit above the ordinary zero.	Pressures in Inches of Mercury according to		Differences between Calculation and Experiment in Inches of Mercury.	Corresponding Differences of Temperature.
	The Formula.	Dr Ure's Experiments.		
*34°	6·20	6·20	0·00	0°·0
44	8·02	8·10	−0·08	+0·4
54	10·24	10·30	−0·06	+0·2
64	12·94	13·00	−0·06	+0·2
*66·7	13·76	13·76	0·00	0·0
74	16·19	16·10	+0·09	−0·2
84	20·06	20·00	+0·06	−0·1
94	24·64	24·70	−0·06	+0·1
*104	30·00	30·00	0·00	0·0
(1.)	(2.)	(3.)	(4.)	(5.)

The results of Dr Ure's experiments on the vapours of *turpentine* and *petroleum*, are so irregular (as the diagram shews), and the range of temperatures and pressures through which they extend so limited, that the value of the constant γ cannot be determined from them with precision. I have, therefore, endeavoured to represent the elasticities of those two vapours approximately by the *first two terms* of the formula only, calculating the constants from two experimental data for each fluid. The equation thus obtained

$$\text{Log P} = \alpha - \frac{\beta}{t}$$

is similar in form to that of Professor Roche.

The data, and the values of the constants, are as follows:—

Temperature on Fahrenheit's Scale from		Pressures in Inches of Mercury.	Values of the Constants for Fahrenheit's Scale and Inches of Mercury.
the ordinary zero.	the absolute zero.		
°	°	*Turpentine.*	
360	822·3	60·80	$\alpha = 5{\cdot}98187$
304	766·3	30·00	Log $\beta = 3{\cdot}5380701$
		Petroleum.	
370	832·3	60·70	$\alpha = 6{\cdot}19451$
316	778·3	30·00	Log $\beta = 3{\cdot}5648490$

Although the temperatures are much higher than the boiling point of water, I have not endeavoured to reduce them to the scale of the air-thermometer, as it is impossible to do so correctly, without knowing the nature of the glass of which the mercurial thermometer was made.

The diagram shews that the formula agrees with the experiments as well as their irregularity entitles us to expect. The following Tables give some of the numerical results.

Table VI.—*Vapour of Turpentine.*

Temperatures in Degrees of Fahrenheit, from the ordinary zero.	Pressures in Inches of Mercury according to		Differences between Calculation and Experiment in Inches of Mercury.	Corresponding Differences of Temperature.
	The Formula (of two terms).	Dr Ure's Experiments.		
°				°
*304	30·00	30·00	0·00	0·0
310	32·52	33·50	− 0·98	+ 2·3
320	37·09	37·06	+ 0·03	− 0·0
330	42·16	42·10	+ 0·06	− 0·1
340	47·78	47·30	+ 0·48	− 0·9
350	53·98	53·80	+ 0·18	− 0·3
*360	60·80	60·80	0·00	0·0
362	62·24	62·40	− 0·16	+ 0·0

Table VII.—*Vapour of Petroleum.*

Temperatures in Degrees of Fahrenheit, from the ordinary zero.	Pressures in Inches of Mercury according to		Differences between Calculation and Experiment in Inches of Mercury.	Corresponding differences of Temperature.
	The Formula (of two terms).	Dr Ure's Experiments.		
°				°
*316	30·00	30·00	0·00	0·0
320	31·71	31·70	+ 0·01	− 0·0
330	36·35	36·40	− 0·05	+ 0·1
340	41·52	41·60	− 0·08	+ 0·2
350	47·27	46·86	+ 0·41	− 0·7
360	53·65	53·30	+ 0·35	− 0·5
*370	60·70	60·70	0·00	0·0
375	64·50	64·00	+ 0·50	− 0·7
(1.)	(2.)	(3.)	(4.)	(5.)

I have also endeavoured, by means of the first two terms of the formula, to approximate to the elasticity of the vapour of *mercury*, as given by the experiments of M. Regnault. The data and the constants are as follows:—

Temperatures in Centigrade Degrees from		Pressures in Millimètres of Mercury.	Values of the Constants in the Formula	
the Freezing point.	the Absolute zero.		Log $P=\alpha-\frac{\beta}{t}$	
°	°			
358	632·6	760·00	α for millimètres =	7·5305
			,, for English inches	6·1259
177·9	452·5	10·72	Log β Centigrade scale	3·4685511
			,, Fahrenheit's scale, boiling point adjusted at 29·922 inches, . .	3·7238236

The following table exhibits the comparative results of observation and experiment.

Table VIII.—*Vapour of Mercury.*

Temperatures in Centigrade Degrees from the Freezing Point.	Pressures in Millimètres of Mercury according to		Differences between Calculation and Experiment in Millimètres.
	The Formula (of two terms).	M. Regnault's Experiments.	
°			
72·74	0·115	0·183	−0·068
100·11	0·480	0·407	+0·073
100·6	0·49	0·56	−0·07
146·3	3·49	3·46	+0·03
*177·9	10·72	10·72	0·00
200·5	21·85	22·01	−0·16
*358·0	760·00	760·00	0·00

The discrepancies are obviously of the order of errors of observation, and the formula may be considered correct for all temperatures below 200° C., and for a short range above that point. From its wanting the third term, however, it will probably be found to deviate slightly from the truth between 200° and 358°; while above the latter point it must not be relied on.

I have not carried the comparison below 72°, because in that part of the scale the whole pressure becomes of the order of errors of observation.

In conclusion, it appears to me that the following proposition, to which I have been led by the theoretical researches referred to at the commencement of this paper, is borne out by all the experiments I have quoted, especially by those of greatest accuracy, and may be safely and usefully applied to practice.

If the maximum elasticity of any vapour in contact with its liquid be ascertained for three points on the scale of the air-thermometer, then the constants of an equation of the form

$$\text{Log } P = \alpha - \frac{\beta}{t} - \frac{\gamma}{t^2}$$

may be determined, which equation will give, for that vapour, with an accuracy limited only by the errors of observation, the relation between the temperature (*t*), *measured from the absolute zero* (274·6 *centigrade degrees below the freezing point of water*), *and the maximum elasticity* (P), *at all temperatures between those three points, and for a considerable range beyond them.*

Some Remarks on the Claims to the Discovery of the Composition of Water.

By JOHN DAVY, M.D., F.R.S., Lond. and Edin., Inspector-General of Army Hospitals, &c. (Communicated by the Author.)

In editing the collected works of my brother Sir H. Davy, I had occasion to make some observations on the above subject, which, at that time (1839), had a good deal of attention given to it, in consequence of the claims then brought forward by the friends of Mr Watt, in favour of the merit of the discovery of the composition of water being due to him alone, to the disparagement of Mr Cavendish, whom the most zealous of those friends evidently wished to exhibit as a plagiarist, or as having derived the idea of the composition of water from Mr Watt, without acknowledgment.

In a work, published in 1846, by J. P. Muirhead, Esq., entitled, "Correspondence of the late James Watt, on his Discovery of the Theory of the Composition of Water," this intention is most fully displayed, and in a manner, it appears to me, characteristic of the advocate—the special pleader rather than of the man of science—in the manner of one anxious to gain a verdict in favour of his client, rather than intent on dispassionate inquiry; and often, in accordance with forensic practice, using damaging expressions of an offensive kind, which never would have been employed in liberal and courteous discussion.

It is not my intention to enter into any lengthened commentary on this work (a quarto of 391 pages). After having carefully read it, I have found no reason to alter the opinion which I had previously formed and expressed, viz. that Mr Watt and Mr Cavendish independently arrived at the idea, or inference, that water is the compound it is now considered, —a conclusion, I have said, alike honourable to Mr Watt and to Mr Cavendish, and which is free from all the difficulties and painful consequences connected with the contrary.*

Such was my first impression, and thus has it been confirmed. The facts on which it was founded are principally the following, as then stated:—Dr Priestley, in his paper "On the seeming Conversion of Water into Air," bearing date Birmingham, April 21, 1783, distinctly mentions "an experiment of Mr Cavendish, concerning the reconversion of air into water, by decomposing it, in conjunction with inflammable air,"† a result which he confirmed by repetition. This result, derived or learnt from Dr Priestley, was the basis, it would appear, of Mr Watt's hypothesis respecting the nature of water, as stated by him to M. de Luc, and also the true basis of that hypothesis, as given in his earlier letter to Dr Priestley.‡

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* Collected Works of Sir H. Davy, vol. vii., p. 133.

† Dr Priestley's words are,—"Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr Cavendish's, concerning the reconversion of air into water, by decomposing it in conjunction with inflammable air."—*Phil. Trans. for* 1783, p. 426.

‡ Mr Watt, referring to Dr Priestley's experiment on the firing of a mixture of dephlogisticated air (oxygen) and inflammable air, and the production of moisture, states, in a note,—"I believe that Mr Cavendish was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired." Mr Caven-

This, his first letter on the subject, was written in the same month as Dr Priestley's paper, that referred to above, and, as will be mentioned further on, was quoted by Mr Cavendish. It was dated April 26, 1783. From a passage in Dr Priestley's paper, and from one in Mr Watt's first letter, that to Dr Priestley, it may be inferred that this his hypothetical conclusion was formed just before that letter was written. He mentions in it the abandonment of an opinion that he had entertained for many years "that air was a modification of water," "that, by a great heat, water might be converted into air."

Now, what is Mr Cavendish's statement relative to the discovery in question? After describing his experiments in proof of the production of water by burning hydrogen in close vessels with common air and dephlogisticated air, he remarks, "All the foregoing experiments on the explosion of inflammable air with common and dephlogisticated air except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr Priestley, who, in consequence, made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions. During the last summer also, a friend of mine gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs should be converted into water."†

It has been objected to this passage that it was an interpolation, after Mr Watt's letter to M. de Luc had been read

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dish obtained 135 grains of water, "pure water," as it seemed, in one of the experiments which he mentions. In Waltire's experiments, which led to Mr Cavendish's, a dew was observed on the inside of the vessel in which the explosion was made, mixed with soot, attended with a loss of weight. The experimenter referred the dew to moisture previously existing in and deposited from the airs used.

* Phil. Trans. for 1784, p. 134.

at the Royal Society, and farther, that it was in the handwriting of Mr Cavendish's friend, Sir Charles Blagdon. That letter to M. de Luc it was, no doubt, which gave rise to the explanation contained in the interpolated statement; and the circumstance that it was not in Mr Cavendish's own handwriting in the MS., it seems most natural to infer would not denote that anything unfair was practised. Had Mr Cavendish not been confident that he was acting correctly, had he been performing the part of a plagiarist, he would, it may be presumed have acted with the caution of a plagiarist at the time. The correctness of the statement, it should be remembered, was not impugned, as, if incorrect, it surely ought to have been.

I have spoken of Mr Watt's conclusion of the compound nature of water as an hypothesis, or as an inference from an experiment requiring to be confirmed by further experiments. In that light, he himself evidently first viewed it; thus, in his letter to Dr Priestley, of the 21st of April 1783, he says, "On considering your very curious and important discoveries on the nature of phlogiston and dephlogisticated air, and on the conversion of water into air, and *vice versa*, some thoughts have occurred to me on the probable causes of these phenomena, which, though they are mere conjectures, seem to me more plausible than any I have heard on the subject, and, in that view, I have taken the liberty to communicate them to you." And he concludes the same letter with this remark,—"If you shall think that a hypothesis so hastily compiled, deserves to have the honour of being communicated to the Royal Society, or published in any other way, with the account of your experiments, I shall be obliged to you to present it to the Society, or to the public, as you shall think proper." Again, in his letter to Sir Charles Blagdon respecting the publication of his paper (his letter to M. de Luc), he says,—"I am really at a loss what title to give the paper; but propose the following,—conjectures—Thoughts on the constituent parts of water, and of dephlogisticated air, with an account of some experiments on that subject." And, in his letter to M. de Luc, he prefaces it with the remark, "I feel much reluctance to lay my thoughts on these subjects (the probable causes of the production of water from the deflagration of a mixture of dephlogisticated and inflammable air) before the public, in their present indigested state, and without being able to bring them to the test of such experiments, as would confirm or refute them, and should therefore have delayed the publication of them until these experiments had been made, if you, Sir, and some other of my philosophical friends, had not thought them as plausible as any other conjectures which have been formed on the subject; and that, though they should not be verified by further experiments, or approved by men of science in general, they may, perhaps, merit discussion, and give rise to experiments which may throw light on so important a subject," adding "I first thought of this way of solving the phenomena, in endeavouring to account for an experiment of Dr Priestley's, wherein water appeared to be converted into air, and I communicated my sentiments in a letter addressed to him, dated April 26, 1783, with a request that he would do me the honour to lay them before the Royal Society; but as before he had an opportunity of doing me that favour, he found, in the prosecution of his experiments, that the apparent conversion of water into air, by exposing it to heat in porous earthen vessels, was not a real transmutation, but an exchange of the elastic fluid for the liquid, in some manner not yet accounted for; therefore, as my theory was no ways applicable to the explaining these experiments, I thought proper to delay its publication, that I might examine the subject more deliberately, which my other avocations have prevented me from doing to this time."

Mr Watt's paper in the Transactions of the Royal Society, bearing the date of November 26, 1783, and which was read the 29th April 1784, consisted, it must be remembered, of portions of his original letter to Dr Priestley, and of additions, some of which, it may be inferred, were made shortly before it was read, viz., in the "corrected copy,"* and when he had a knowledge of the experiments both of Mr Cavendish and of Lavoisier and La Place on the composition of water. In his published paper, he says, "I am obliged to your friendship (De Luc's) for the account of the experiments which have been lately made at Paris on this subject, with large quantities of the two kinds of air, by which the essential point seems to be clearly proved, that the deflagration or union of dephlogisticated and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water thus produced appeared to be pure water." M. de Luc's letter, conveying this information, is of the 9th of February 1784.

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* It is designated, in Mr Watt's handwriting, "Corrected copy of a letter from James Watt, Engineer, to M. de Luc, dated November 26, 1783, corrected April 1784." It is in the Archives of the Royal Society.

Mr Watt, it would appear from this his published statement, was induced by his friends to bring forward his views on the composition of water. The friend who was most active on this occasion, judging from the correspondence, was M. de Luc, who seemed to take a pleasure in exciting Mr Watt against Mr Cavendish. His letter of the 2d of May, finished on the 4th, is an example of the kind, in which he raises the suspicion of plagiarism on the part of Mr Cavendish,* and to which, in reply, Mr Watt observed, "I mean to be in London next week, where your demands on my person shall be answered, and to which time I must refer particulars, having much business, but of another nature than the plagiarism of Mr C., pressing hard upon me. On the slight glance I have been able to give your extracts of the paper,† I think his theory very different from mine; which of the two is right I cannot say; his is more likely to be so, as he has made many more experiments, and has, consequently, more facts to argue upon. I by no means wish to make any illiberal attack on Mr C. It is *barely* possible he may have heard nothing of my theory," adding, "as to what you say of making myself '*des jaloux*,' that idea would weigh very little; for were I convinced I had foul play, if I did not assert my right, it would either be from a contempt of the modicum of reputation which could result from such a theory, or from a conviction in my own mind that I was their superior, or from an indolence that makes it easier to me to bear wrongs than to seek redress. In point of interest, in so far as is connected with money, that would be no bar; for though I am dependent on the favour of the public, I am not on Mr C. or his friends, and could despise the united power of the '*illustrious house of Cavendish*,' as Mr Fox calls them." He adds, "you may, perhaps, be surprised to find so much pride in my character. It does not seem very compatible with the diffidence that attends my conduct in general. I am diffident, because I am seldom certain that I am in the right, and because I pay respect to the opinions of others where I think they may merit it. At present, *Je me sens un peu blessé;* it seems hard, that in the first attempt I have made to lay anything before the public, I should be thus anticipated. It will make me cautious how I take the trouble of preparing anything for them another time. I defer coming to any resolution till I see you; but at present I think reading the letter at the Royal Society to be the proper step."*

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* Correspondence, p. 48.

† Sent by Mr Cavendish, in MS., to M. de Luc, at the request of the latter.

M. de Luc, in reply to this letter, writing on the 10th April 1784, desires to have a corrected copy of Mr Watt's paper retaining the original date. His words are:—"Mais si vous souhaitez que ces lettres soient lues, envoyez moi d'avance la nouvelle edition (the corrected copy?) de celle que vous m'aviez ecrite le 26 *Novembre;* en y mettant la même date; afin que la traduction que j'en ferai, soit d'accord avec ce qui sera lu à la Societé." This advice he followed, as would appear from a letter from him to Sir Joseph Banks, of the 17th April, in which he says, after alluding to certain alterations and additions,—"I thought it right to apprise you of these alterations, lest it should be said by any body that the letter was fabricated at a later date than it bears."

Judging from the letters which are given in the correspondence of Mr Watt and Sir Joseph Banks, then President of the Royal Society, and of Sir Charles Blagdon, the Secretary of the Society, the most courteous attention was paid by them to Mr Watt on the matter under consideration, with an earnest desire to publish his papers in the Transactions, and to meet his wishes as to the manner of bringing forward his views on the composition of water,—views, it is manifest, which gained importance after the careful experiments had been made, and made known by Mr Cavendish, and after him by Lavoisier and La Place.

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* Correspondence, p. 49.

For many years, it is deserving of remark, the honour of the discovery of the composition of water was divided between Mr Watt and Mr Cavendish,—or rather, I should say, given to each undivided,—to Mr Watt for the happy thought or conjectures ("Conjectures on the constituent parts of water")—to Mr Cavendish for the experiments and inferences from them in proof of the composition of water.*

Lord Brougham, in commenting on some remarks made by the Rev. Vernon Harcourt in support of Mr Cavendish as an independent discoverer, not a plagiarist, observes,—"It may be as well to repeat the disclaimer already very distinctly made, of all intention to cast the slightest doubt upon that great man's perfect good faith in the whole affair; I never having supposed that he borrowed from Mr Watt, though M. Arago, Professor Robison, and Sir H. Davy, as well as myself, have always been convinced that Mr Watt had, unknown to him, anticipated his great discovery."† This, probably, will be the decision of posterity; and, as I have before remarked, it is the one equally honourable to the two distinguished men concerned, whose reputations should be as cherished and dear to us—if gratitude is not a mere word,—as their labours have been glorious and above all praise. Is there not a mistake and a meanness in endeavouring to detract from the one, to add, if that be possible, to the reputation of the other?

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* What, it may be asked, constitutes a discovery? Is not Sir John Herschel right in the principle, that "discovery consists in the certain knowledge of a new fact, or a new truth, a knowledge grounded on positive and tangible evidence as distinct from bare *suspicion* or *surmise* that such a fact exists, or that such a proposition is true;"—and is he not right as to the time of a discovery, that it is when the discoverer is first enabled to say to himself or to a byestander, "*I am sure that such is the fact;*" and I am sure of it, "*for such and such reasons.*"—*Address to the Roy. Astron. Soc. in Edin. New Phil. Journal,* vol. xlvi., p. 256.

† *Historical note by Lord Brougham.* Correspondence, p. 256.

It may appear late, on my part, to offer these remarks, considering that the work which has called them forth has now been nearly four years before the public. My absence from England, in the West Indies, during the interval, and my want of knowledge, in consequence, of the arguments used against Mr Cavendish, require only to be noticed to account for it.

Lesketh How, Ambleside,
April 24, 1849.

On the Acid Springs and Gypsum Deposits of the Upper Part of the Silurian System (Onondaga Salt Group). By T. S. Hunt, of the Geological Survey of Canada.

That portion of the upper Silurian system of New York, which has been designated by the geologists of that state the Onondaga Salt Group, is characterised not only by the saline springs to which it owes its name, but also by numerous deposits of gypsum and springs, which are sour to the taste, and contain free sulphuric acid. The one at Byron, New York, has long been known, and several others have been observed more recently in the same geological district. The same region in Canada affords a remarkable spring of this kind, which, in the course of my official duties, I had occasion to examine in the month of October 1847. It is situated in the township of Tuscarora, in the Indian Reserve, about twenty miles north of Port Dover, which is the nearest point on Lake Erie. The water contains a large amount of free sulphuric acid, about 4 parts in 1000, besides sulphates of the alkalies, lime, magnesia, aluminum, and iron in small quantities. The proportion of these ingredients is however not constant, as is evident from an analysis made in April 1846, by Professor Croft of King's College, Toronto, which is confirmed by a partial examination by myself, of a specimen of water brought from the spring in June 1845.

The specific gravity of the water was much lower, and the amount of foreign ingredients much less, than in that collected by myself, but the proportion of bases to the acid was much greater. The proportion of the lime to the acid I found to be about 1·15, and that of the magnesia 1·90, while Professor Croft's determination gave about 1·6, and one to 1·15, respectively. That collected in 1845 is a nearly saturated solution of gypsum, while that of 1847 contains no more than about 7 parts in 10,000.

These facts indicate a rapid change in the constitution of the spring, and its situation shews it to be of comparatively recent origin; for although the powerful acid has destroyed all traces of vegetation for a distance of several yards around the source, the water issues from beneath the roots of an enormous pine-tree, whose decayed stump still stands several feet in height, while the crumbling mould, from its slow decay, forms the surface soil for some distance around. Without overlooking the antiseptic virtues of the mineral substances contained in this remarkable spring, this fact shews that its antiquity can scarcely be greater than a century, if indeed half that cycle may not extend beyond the time of its first development, while the rapid decrease in the quantity of the saline bases shew that its character remains constant scarcely for a twelvemonth. It should have been observed, that there are four or five basins within the distance of as many yards, and that they are situated on the summit of a low mound, which rises with a gradual slope from the marshy plain.

The probable cause of these changes will be seen by adverting to the character of the gypsum deposits of this formation, which I regard as having an intimate connection with this class of springs. The investigations of Mr Hall, in New York, and Mr Murray, in Canada, unite in shewing that the gypsum of these rocks always occurs in hillocks or dome-shaped masses, varying in size from one foot to 300 or 400 feet in diameter, and always near the surface of the formation. Sections of these masses shew them resting upon undisturbed strata of limestone, while the superior strata are thrown up and rest upon the flanks of the intruded hillock, often very much broken, and, as Mr Hall has remarked, in part consumed, so that one is at a loss to account for the disappearance of a large portion of the overlying strata. In one case observed by Mr Murray, a slender cylinder of gypsum passes through several beds of the limestone, and at last terminates in a cone of the usual form, which is entirely superior to the limestone formation and surrounded by the tertiary clay of the region. The comparatively recent origin which this assigns to the gypsum deposits, is confirmed by the common experience of the people of Western New York, where it is a well known fact that since the settlement of the country, walls have been disturbed and houses raised from their foundations by a gradual elevation of the surface, where subsequent examination has shewn the presence of domes of gypsum.

On comparing these facts with those observed in connection with the acid spring, it appears probable that the origin of the gypsum is to be ascribed to the action of such mineral waters upon the calcareous strata. How far the pressure at a great depth may operate in preventing chemical changes, we may not know, but it is easy to see that once coming to a situation where it could act upon the limestone, it would evolve carbonic acid gas, and form a calcareous sulphate which from its sparing solubility would be at once deposited in a crystalline form, while the water would pass off saturated with the sulphate, and at the same time carrying with it the soluble sulphates of magnesia, alumina, and iron, which would be formed from the other bases generally present in the limestones of this region. If the amount of acid were copious, or the supply of calcareous matter limited, the water might rise to the surface with free acid, as in the cases already noticed, and when the deposition of calcareous sulphate had extended so far as to protect the carbonate from farther action, the water would appear with a much smaller portion of basis than before, having only the sulphate of lime, which it could dissolve from the sides of its channels.

If, on the contrary, the acid were entirely neutralized, the spring would present at the surface the character of an ordinary bitter saline, containing calcareous and magnesian sulphates ; two springs of this character are indeed found in the same formation not far from here. The ferruginous and argillaceous substance known as *gypsiferous marl*, which surrounds these deposits, seem to be due to the precipitation by the carbonate of lime, of the iron and alumina, which have been previously taken up by the water yielding a mixture of these oxides with carbonate and sulphate of lime. The fact that crystalline gypsum occupies nearly twice the space of an equivalent quantity of carbonate of lime, will at once explain the displacement of the superincumbent materials. The observation which is now required to confirm this theory, is to find the carbonic acid which should be evolved from the decomposition of the limestone actually disengaged from one of these springs ; the small quantity of gas which rises from the Tuscarora spring was found to be principally carburetted hydrogen, which is copiously evolved by the salines of this region, but it was collected at a time, when, from the minute portion of gypsum in the water, the action seems to have been at an end. I shall not attempt to speculate upon the probable source of the sulphuric acid at present, but shall defer the consideration of the subject, until the publication of my report on the mineral springs of Canada, which will be accompanied with the analyses of this water as collected in different years. Hoping that my observations may resolve a hitherto unexplained problem in the geology of this region, I beg leave to submit them to the notice of the Association.—*American Journal of Science and Arts*, vol. vii., No. 20, New Series, p. 175.

An Account of Two Aërolites, and a Mass of Meteoric Iron, recently found in Western India. By HERBERT GIRAUD, M.D., Professor of Chemistry in the Grant Medical College, Bombay, Assistant-Surgeon in the Hon. E.I.C.'s Bombay Medical Establishment. Communicated by the Author.

Although the records of science have of late years abounded in descriptions of meteorites, yet, from their unearthly origin, and characteristic chemical composition, these curious bodies continue to claim the peculiar interest which, it is believed, may attach to two specimens of aërolites, and one of meteoric iron, that have recently been found in Western India.

Aërolite from Dharwar.—About one o'clock P.M. of the 15th of February of last year, this aërolite fell in a field to the south of Negloor, a village situated within a few miles of the junction of the Wurda and Toomboodra rivers, and belonging to the Gootul division of the Ranee-Bednoor Talook of the Dharwar collectorate.

The fall of this aërolite is most satisfactorily established. A cultivator of Negloor, named Ninga, was driving his cattle out to graze close by where it fell; at the hour above mentioned, he suddenly heard a loud, whirring, rushing noise in the air, but on looking up could see nothing. An instant afterwards, however, he observed a cloud of dust rise from a spot in an adjoining field, as if something had struck the ground there with violence. At this time several other villagers were standing by a threshing-floor, close at hand; they also heard the noise, and called out to Ninga, asking whether he had done so. He replied, Yes, and that something had fallen in the next field, where he saw the dust rise; and on his pointing to the spot, the whole party proceeded thither. There they found a stone, broken into fragments, imbedded in a hole, which they compared to the print of a young elephant's foot. They were naturally much puzzled to account for the appearance of the stone, which altogether differed from any to be met with in their neighbourhood, and at length they were constrained to conclude that it had fallen from the heavens. The circumstance seemed so extraordinary, that one of them was immediately sent to summon the Patel of the village, who soon arrived, attended by a crowd of people, who had also heard the wonderful tidings. These, too, unanimously adopted the same conclusion regarding the fall of the stone, the fragments of which the Patel took into his charge, and wrote a report of the whole circumstances to the Mahulkarree of Gootul, who is the revenue and police officer of the district in which Negloor is situate. The Mahulkarree thought the Patel's report so extraordinary, that he determined at once to proceed to Negloor himself to inquire into its truth. After having examined the stone itself, and the hole in the ground made by its fall, and finding that all the accounts of the villagers agreed, he could not avoid concluding, as they did, that it fell from the sky. He, moreover, took statements in writing from the cultivator Ninga and another, who had heard the rushing noise made by the stone in its passage through the air, and forwarded their depositions, with his own reports, and the fragments of the aërolite, to Mr Goldfinch, the the assistant-collector and magistrate in charge of the district, by whom they were given to Captain G. Wingate, of the Bombay Engineers, who presented them to the Bombay Geographical Society, and by the secretary to this institution the fragments of the stone were made over to me for examination and analysis.

The fragments of the stone being placed together, constituted a *mass of an ovoidal figure,* measuring 15 inches round the larger, and

11 round the shorter axis, and weighed 4 pounds. One end of it was somewhat flattened, as if it had, whilst in a soft state, come in contact with some hard substance. Its whole surface was covered with a blackish, vitrified-looking crust, about one-twentieth of an inch in thickness, whilst the interior resembled a greyish-white soft sandstone, diffused through which were minute brilliant metallic particles, about the size of small pin-heads. It crumbled readily under the fingers; and when reduced to powder, the metallic particles could all be abstracted from it by a magnet. Its specific gravity was 3·512. Hydrochloric and nitric acid acted violently upon it, with evolution of sulphuretted hydrogen gas, and solution of the metallic particles.

Its analysis was conducted by digesting it with heat, in nitrohydrochloric acid, leaving the insoluble earthy silicates: precipitating the iron as peroxide by an excess of ammonia, which gave a pale sapphire-blue solution: then evaporating to dryness, igniting to expel the ammonia salt: dissolving the residue in nitric acid, and precipitating the nickel as oxide by potash. Neither cobalt nor chromium were detected by qualitative methods. After the action of nitro-hydrochloric acid, sulphur floated on the liquid, and, moreover, much sulphur was given off as sulphuretted hydrogen.

COMPOSITION.	
Earthy silicates, ...	58·3
Sulphur, ...	2·5
Nickel, ...	6·76
Iron, ...	22·18
	89·74

Aërolites, from the Myhee Caunta.—On the 30th of November 1842, at 4 P.M., some Khoonbees were sowing grain between the villages of Jeetala and Mor Monree, in the Myhee Caunta, to the north-east of the city of Ahmedabad, when they heard a noise or report like the firing of heavy guns, four or five times; this sound came from the east, and was instantly followed by a violent gale of wind, and the fall of a number of stones,—of these the Khoonbees picked up one that fell on the edge of their field; it weighed about ——.* When first taken up, it smelt strongly of gunpowder. The people broke it to pieces, and kept them as curiosities. One of the fragments having fallen into the hands of a Karkoon, he brought it to Captain G. Fulljames, Commandant of the Goozerat Irregular Horse, who transmitted a small portion of the stone to the Bombay Geographical Society. This fragment presented so exactly the appearance of the foregoing aërolite from Dharwar, that it might have been taken for a portion of it; presenting the same dark vitrified surface, the greyish-white siliceous interior, with the brilliant metallic particles diffused through it. Its specific gravity was somewhat less than that of the preceding aërolite, being 3·360. The portion which was placed in my hands for analysis, was unfortunately too small to afford other qualitative results; these, however, pointed to its close resemblance to the Dharwar stone, for with the earthy silicates it contained sulphur, iron, and nickel.

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* *Professor Giraud's MS.* leaves blank space in place of weight.—Edit.

Meteoric iron from Singhur, near Poona in the Deccan.—The hill fort of Singhur has, of late years, during the hot season, become a favourite resort of European officers, stationed at Kirkee and Poona, from which latter place it is about fourteen miles distant. The fort, situated upon a basaltic hill, is at an elevation of about 2000 feet above the surrounding plain, and 4500 above the level of the sea.

In November 1847, as some workmen were improving the ascent to the fort, they stumbled upon a mass of what they supposed to be iron ore, lying upon the surface of the ground; but from its being so totally unlike any rock in the neighbourhood, they took it as a curiosity to the Rev. Mr Reynolds, the chaplain of Kirkee, who was at the time residing at Singhur. Mr Reynolds, struck with its singular locality and appearance, transmitted it to Dr Buist, the secretary to the Bombay Geographical Society, from whom I received it for examination.

The mass is of an irregular three-sided prismatic form, tapering and conical at the ends. It is 12½ inches long; and at its broadest parts the sides are from 5 to 5½ inches across. It weighs 31 lb. 4 oz. The specific gravity of the several pieces that have been detached from the mass, varies from 4·720 to 4·900. The whole surface is of ferruginous colour, with here and there bright metallic-looking portions, of the colour and appearance of malleable iron. One of its sides is highly vesicular, as if gases had been extricated from it, whilst solidifying from a state of fusion; another of its sides is less vesicular than this; and the third is flattened and metallic-looking, as if it had been beaten with a sledge-hammer, or had fallen while soft upon a hard surface. On boring into the mass for the purpose of obtaining portions for analysis, it was found to have large irregular vesicular-surfaced cavities in its interior, and the walls of these, as well as the borings (which were powdery), were of a deep-slate colour, or almost black.

On having a portion of one of its extremities cut off, small, yellowish-white, earthy-looking bodies, about the size of peas, were observed sparingly scattered through and embedded in the iron. The mass is so exceedingly tough, that portions could not be detached from it by the hammer, and it was found necessary to heat it before a piece could be cut from it. It is malleable, powerfully attracts the magnet, but has no magnetic poles, as some masses of meteoric iron have been found to possess. The analysis of the borings, taken from a depth of three inches, gave of—

Earthy silicates, ...	19·5
Iron, ...	69·16
Nickel, ...	4·24
	92·93

The strict resemblance of this specimen, both in physical and chemical properties, to the many recorded examples of meteoric iron, leave no doubt regarding its nature.

Its vesicular surface indicates a state of fusion, which the power of the native furnaces of this country is quite inadequate to produce in iron of such toughness and malleability; and, moreover, its constituent nickel, so near the average proportion of five per cent., points distinctively to its meteoric origin.

Like the Siberian meteoric iron, described by Pallas, when heated strongly, it became brittle, refused to extend under the hammer, and broke into grains; and, like the Brazilian specimen described in the Philosophical Transactions, it gave abundance of sparks when struck with a steel hammer.

M. ALCIDE D'ORBIGNY *on Living and Fossil Molluscs.*

Among the zoologists whose labours have contributed most to increase our acquaintance with the relations which ancient faunas bear to the animals of the present epoch, M. Alcide d'Orbigny occupies a place in the first rank. This skilful naturalist has published, within these few years back, a series of works,* which, taken together, may be said to form an epoch in the history of zoology and palæontology. Studying each natural group of the great and important class of mollusca in succession, and comparing these animals in the different geological periods, and in the existing world, he has reached results of the highest interest. We shall here explain the most important of these, selecting more especially such of them as are connected with those principles of palæontology which we have often had occasion to lay before our readers.

We shall first bring forward the following considerations on the geographical distribution of living molluscs. No one ever enjoyed better opportunities of studying these animals in every point of view, than M. d'Orbigny. His early years were spent on the shores of the ocean, while a journey of seven years' duration in South America, and immense collections, have furnished him with numerous points of comparison.

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* M. d'Orbigny's works of which we chiefly wish to speak, are his *Paleontologie Française* (already consisting of upwards of 170 livraisons,) which is devoted to the fossil molluscs of France; his *Paleontologie Etrangère*, the companion of the former; his History of Living and Fossil Molluscs, a work which will be of immense utility, if it be completed. He promises, besides, a *Cours de Paléontologie Generale et Appliquée.*

"The geographical distribution of molluscs is of great importance, because, proceeding from the known to the unknown, it is calculated to make known to palæontology, by the laws which regulate the geographical distribution of living beings, what has taken at the different epochs of animals appearing on the globe. I shall here mention, in a general way, some of the principal results with which my numerous investigations on this subject have already furnished me.

"The study of terrestrial animals has proved to me that the species, restricted by limits more or less extensive, were distributed each according to special* zones of temperature, complicated, nevertheless, by influences determined by the orographic form of continents and their phytographic composition. In general, the number of species decreases in proportion as we recede from the warm regions and approach the cold regions.†

"The study of marine pelagic animals, or such as belong to deep seas, has in like manner demonstrated to me that of the cephalopods,‡ notwithstanding the number of species which pass indifferently from one ocean to another, more than two-thirds of each sea are peculiar to it. These numbers evidently prove, that the limits of fixed habitation still exist in respect to animals, which their power of locomotion, and pelagic habits, would distribute throughout every sea, if Cape Horn on the one hand, and the Cape of Good Hope on the other, were not, in their southern position, altogether beyond the torrid zone, which nearly all the species inhabit, and thus form a barrier which they are unable to pass. We have, therefore, the certainty that uniformity of temperature, more than any other agents, is the true basis of the geographical distribution of the animals of the high seas. We may add, that they are found to be more complicated in their forms, and more numerous in species, the nearer we approach the warm regions. The pteropods, although more indifferent as to temperature, have afforded me the same general results,* with respect to their geographical distribution in the oceans.

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* See my observations on this subject, *Mollusques de mon Voyage dans l'Amerique Meridionale,* p. 215.

† Same work.

‡ Memoir read to the Academy of Sciences, 19th July 1841, and inserted in the *Monographie des Céphalopodes Acétabulifères.* Introduction.

"The investigations which I have in like manner undertaken, although much more difficult, in order to become acquainted with the laws which regulate the geographical distribution of the molluscs of sea-coasts, have led me to curious results.† I have ascertained, for example, the action of three different influences,—currents, temperature, and the orographical configuration of coasts.

"We thus perceive, that if currents, by their long-continued action, have a tendency to spread the molluscs of coasts beyond their natural limits of latitude, when they carry them to a distance from a continent, or round a cape advanced in the direction of the pole,—or when they suddenly leave the coasts under the warm regions, we must ascribe to them, on the other hand, the isolation and establishment of local faunas.

"I have likewise ascertained that, notwithstanding the active influence of currents, the passive action of heat is everywhere felt in a very marked manner, by forming collections of species in more or less restricted limits of latitude.

"The orographical configuration of the coasts of oceans, by offering conditions of existence more or less favourable to littoral molluscs, according to their genera, exercises also immense influence on the zoological composition of the faunas which inhabit them.

"From the combined effect of these three kinds of influences we may infer, with certainty, that the laws which regulate the geographical distribution of coast molluscs, may be reduced to two contrary actions,—currents which have the tendency to spread, wherever they pass, the species indifferent to temperature; currents, temperature, and orographical configuration, which tend, on the contrary, to restrict and localise beings within limits more or less extensive.

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* Memoir read to the Academy of Sciences in 1835, and inserted in the molluscs of my *Voyage dans l'Amerique Meridionale*, p. 68.

† See my Memoir, laid before the Academy of Sciences in November 1844, and printed in 1845 in the *Annales des Sciences Naturelles*.

"I can further deduce from my researches the following conclusions, which are interesting in a palæontological point of view:—

"Two neighbouring seas, communicating with each other, but separated only by a cape advancing in the direction of the pole, may have their faunas distinct.

"Distinct faunas may exist at the same time, by the sole action of temperature, in the same ocean and on the same continent, according to the different zones of temperature.

"Under the same zone of temperature, and on the neighbouring coasts of the same continent, currents may determine the particular faunas.

"A distinct fauna from the fauna of the nearest continent may exist in an archipelago, when the currents have the effect of insulating it.

"Distinct faunas, or at least very different from each other, may appear on neighbouring coasts, in consequence of the sole influence of orographical configuration.

"When we find the same species over an immense extent of latitude, in the same basin, currents must be regarded as the cause of it.

"Identical species, in two neighbouring basins, indicate direct communication between them.

"The largest tributaries do not absolutely exercise any influence on the composition of the marine faunas of sea-coasts."

These researches on geographical distribution have afforded M. d'Orbigny most valuable data for studying the geological distribution of these same animals.

"After having given a brief view of my investigations relative to the geographical distribution of living molluscs, I ought to say something of the distribution of the species buried in the strata which compose the crust of the earth.

This subject having been equally the object of my special investigations for many years, both in Europe[*] and America,[†] I shall mention some of the results I have obtained up to the present time, until the successive synoptical views, in genera and classes, furnish me with more complete and definite solutions. The following are the conclusions which I can now deduce,—conclusions of great interest for the solution of the important questions respecting the chronological history of animal life on the surface of the earth.

"Molluscs, considered as a whole, have proceeded according to the chronological order of the faunas peculiar to the formations, from the simple to the composite. Many genera, it is true, have completely disappeared with the ancient formations;[‡] others, appearing at a later period,[§] have likewise become extinct with the strata of the cretaceous formations; but the genera, multiplying more and more as we recede from the first age of the world, have been replaced, during the period of the cretaceous and tertiary formations by a multitude of forms which were wanting in the lower beds,[||] and these forms are still more diversified in our present seas,[¶] where they reach the maximum of their numerical development.

"No transition being traceable in the specific forms, molluscs appear to succeed each other on the surface of the globe, not by a gradual passage, but by the extinction of existing races, and the renovation, or successive creation of species at each geological epoch.

"Molluscs are distributed in zones, according to the geological epochs. Each of these epochs, in fact, represents on the surface of the globe a distinct fauna, but identical in its composition; thus the silurian, devonian, and carboniferous stages, the triasic, jurassic, cretaceous and diluvian formations, appear to be the same over the whole earth,* and there preserve the same generic forms, along with the same palæontological *facies*.

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[*] See my *Paleontologie Française*, and particularly the Synopsis at the end of each class, vols. i. ii.

[†] *Paleontologie de l'Amerique Meridionale* (*Voyage dans l'Amerique Meridionale*, vol. iii.) See also the *Géologie* of the same work.

[‡] The Orthoceratites, Cirthoceras, Goniatites, Productus, and Spirifera.

[§] The Ammonites, Toxoceras, Ancycloceras, Ptychoceras, Erioceras, Hamites, Acteonella, &c.

[||] A multitude of genera have appeared at this epoch; Voluta, Mitra, Murex, &c.

[¶] The number of genera not known in a fossil state is a proof of this; Pedum, Magilus, &c.

"Not only is there the same *facies* and the same generic forms in the lost fauna of the whole globe, but there are also some identical species, common everywhere, which prove how completely they are contemporaneous.

"This contemporaneous existence which we remark at immense distances from the first period of animalization,† and even up to the time when the lower cretaceous strata were deposited,‡ seems to depend on a uniform temperature and the shallowness of seas; indeed, such conditions would allow these beings not only to enjoy everywhere the influence of the exterior light, a circumstance indispensable to their existence, but also to propagate and spread themselves without obstruction from one place to another. But this state of things could not be continued after the influence of latitude, and, consequently, the inequality of temperature caused by the cooling of the earth, on the one hand, and, on the other, the elevation of the earth as well as the great depths of the ocean produced barriers which the sedentary zoology of the coasts could not pass beyond. We must, therefore, suppose that the uniformity in the distribution of the earliest beings on the globe is owing as much to the equality of temperature determined by the central heat, as by the shallowness of the seas; while the separation of faunas by basins of greater or less extent, arises, as we approach the existing period, from the cooling of the earth, the limits of latitude, terrestrial barriers caused by continents, and marine barriers occasioned by the depths of the ocean, all of which have presented obstacles to the extension of river and pelagian faunas.

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* I have found it to be the case at least in regard to America and Europe.—*Voyage dans l'Amerique Meridionale*, vol. iii.; *Paleontologie*, p. 175.

† The Productus, the Spirifer, and the species of other genera are found simultaneously in Europe and America.

‡ See my Fossiles de Colombie, 1842, where many species are identical in America and Europe.

"If faunas have the same points of separation on different continents, and if they are arrested by limits strongly defined in their palæontological composition, we must naturally infer that the divisions of the formations do not depend on partial causes, but that they arise from general causes whose influence is felt over the whole earth.

"From my researches in America, where geological facts are observed on a large scale, I am led to believe that the partial or total annihilation of faunas peculiar to each formation, always arises from the importance of dislocations produced on the surface of our planet by the contraction of the matters owing to the cooling of the central parts,* and the perturbations which have produced these same dislocations. A system, or rather a chain of mountains, 50 degrees in length, such, for example, as that of the Andes, of whose relief only we can judge, without being able to calculate the corresponding extent of its sinking in the bosom of the ocean, must have caused such a movement in the waters, in consequence of the displacement of matters, that the effect would be universal both on continents and in seas. By this deluge land-animals have been swept away from the former; the transportation of earthy particles has desolated the second, not only suffocating the animals living at large in the ocean by filling their branchiæ, but also the more fixed animals of the coast, by covering them up under a deposit. We may likewise suppose that a great disturbing cause has resulted from the difference of the levels formed along the whole shore of oceans by this terrestrial movement. We may thus explain, at the same time, the separation of beings by formation, and their extinction at each of the great geological epochs.

"The results of these dislocations being general over the globe, and having manifested themselves at immense distances, we ought to seek in them the systems of elevation or *effect de bascule*, ancient and modern, the cause of the annihilation of the numerous faunas which have succeeded each other on the surface of our planet. When we fail to find, at points in the vicinity of the place where their distinct faunas at present appear, a sufficient explanation of the circumstance from chains of mountains, we must seek it more remotely, in points still unknown to science, or suppose that if these terrestrial systems are the cause, many of them have been destroyed by new sinkings. Chains of mountains, moreover, are only the visible portion of the dislocation of the globe, while the sunk portion, perhaps more considerable, being for the most part covered, is unknown to us, and will always continue to be so.

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* This is the opinion of M. Elie de Beaumont.

" In a word, the separation of stages and formations by distinct faunas, is nothing more than the visible consequence of the varied elevations and sinkings of the earth's crust in all its parts.

" I may further remark, from the uniform distribution of the same beings, that, up to the period of the cretaceous formations,* the internal heat of the earth has destroyed the whole influence of latitude and polar cold. If exterior atmospheric influence on the distribution of beings on the earth's surface did not then exist, all the faunas anterior to the cretaceous formations certainly owe their limitation by formations to the great dislocations of the globe. It would be at a posterior date that the influences of latitude rendered the division into basins more complicated, multiplied the local faunas, as is seen in the tertiary formations, and destroyed that uniformity of distribution which is observed in the most ancient formations.

" Assuming as a basis, the superposition and points of separation more or less decided among the faunas which have succeeded each other, from the first appearance of animals on the globe up to the present time, the following, in the order of their succession, are the formations and stages deduced from geological and palæontological observations.

PALÆOZOIC FORMATION.
First, Silurian stage.
Second, Devonian stage.
Third, Carboniferous stage.
Fourth, Permian stage.
Fifth, Triasic stage.

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* *See my particular work on Coquilles Fossiles de Colombie.*

JURASSIC FORMATION.

First stage, the Lias…………	*Inferior Lias*, belonging to the zone of *Gryphæa arcuata*, and below it. *Middle Lias*, belonging to the zone of *Gryphæa cymbium*, up to *Gryphæa arcuata*. *Superior Lias*, above *Gryphæa cymbium*.
Second, Bathonian stage…….	Lower *Oolite*. Great *Oolite*.
Third, Oxford stage…………	*Inferior Oxford* stage (*Kelloway rock*). *Middle Oxford* stage (*Oxford-clay*). *Superior Oxford* stage (*Coral-rag*).
Fourth, Kimmeridgian stage…	*Inferior*, or Kimmeridgian. *Superior*, or Portlandian.

CRETACEOUS FORMATION.

First stage, Neocomian………	*Neocomian*. *Aptian*.
Second stage, Albian or gault.	
Third stage, Turonian ………	*Turonian*, or chloriteous chalk. *Senonian*, or white chalk.

TERTIARY FORMATION.

First stage, Parisian…………	*Inferior*, to the coarse limestone. *Superior*, or coarse limestone and superior beds.
Second stage, Sub-Appenine.	
Third stage, Diluvian ……….	In this latter period we find none but species now living.

After these general considerations, we may add a few words on the more special facts, the study of which is of direct interest to palæontologists ; and we shall begin with the simple *accidents* of fossilisation in shells.

" The state of preservation in shells may often deceive the observer, and lead him to separate different states of the same shell as distinct species. Shells, whether they have been buried by the strata of the earth in the place where they occur, or have been conveyed by currents, are generally arranged in zones in the fossiliferous formations. According to their geological age, or the longer or shorter period they have remained in these strata, they are completely or partially changed in their nature. Such a shell composed, for example, of molecules of carbonate and phosphate of lime, and horny or mucous animal molecules, sometimes still retains, in its composition, some of the carbonate of lime ; but then this substance, at least if it be not of a lamellar texture, as in the shells of certain genera,* does not preserve its original appearance. The mineral matter by which it is replaced, is formed of carbonate of lime,† silica,‡ sulphuret of iron,§ hydrated iron,‖ oligistic iron,¶ sulphate of strontian,** sulphate of barytes,†† lead,‡‡ or any other substance, no longer possesses its primitive internal texture. It is the mineral matter in its ordinary aspect which occupies the place of the shell. When shells have only changed their nature, they preserve all their characters, and it is easy to study them.

" Shells enveloped in particles of clay, marl, or lime, after their deposition in ancient seas, which have been afterwards entirely destroyed by the action of chemical agents, and left their places empty, present greater difficulties. When the void has remained untouched, it shews on the one side the *impression* of the exterior characters, and on the other that of the internal characters. It is then for the observer to endeavour to reconstruct, by artificial means, or to recognise the character of the genera and species of the shell by the union of the two impressions remaining in the rock. A single valve of the acephales presenting at once the exterior form and the hinges, may admit of pretty easy determination; but it is not always so with the gasteropods, and particularly the bivalves, when they have been shut, and left only what has been improperly called the *kernel* or *interior mould*, which I would designate as the *internal impression;* for then a great number of conchyliogical characters, such as those of the hinges, have often disappeared, and in many cases it is extremely difficult to determine the genera and species. But if the difficulties begin with the internal impressions of entire bivalves pretty well preserved, they increase when the state of preservation becomes still less complete. I refer to *counter-impressions*, when, for example, the shell has completely disappeared, in an argillaceous or calcareous bed in a still unsolidified state; and when the impression produced by the weight of the superior beds, tends to make the bed more compact by bringing all the parts towards each other; then the void left in place of the shell disappears, and the interior and exterior impression, united and brought in contact, sometimes completely attenuate the internal characters, or at least produce an appearance which is neither an internal or external impression, but rather a combination of both. In these circumstances, of very frequent occurrence, the characters are altered, and very difficult to recognise.* It is commonly not till after having handled and seen thousands of fossil shells of this nature, that we can succeed in perceiving, in the most fugitive characters, what must have existed in the primitive state.

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* *Ostrea* and *Terebratula*.

† Such are found in France at an infinite number of places.

‡ All the shells of Uchaux (Vaucluse) and Launoy (Ardennes), contained in the cretaceous and Oxford strata, are in this state.

§ The greater part of the shells at Vaches-Noires (Calvados) are thus transformed.

‖ This transformation is very common.

¶ This is met with in the vicinity of Semur (Côte d'Or), in the lias.

** I have collected some of these in the Neocomian stage, near St Dizier (Hautemarne).

†† I possess belemnites in strontian, discovered by M. Delanoue, in the lias of the neighbourhood of Nontron (Dordogre).

‡‡ I have some gryphées thus transformed, from the neighbourhood of Semur.

" A second cause of error is owing to the disappearance from certain strata of the substance of shells, and the preservation of certain others in the same subjects. This modification, very common in the ancient formations,† is likewise so in the most modern.‡ We observe, for example, the exterior layer of the shell disappear, and along with it the specific characters, leaving a second, which is, for instance, smooth, while the former was striated,* or striated while the first was smooth.† It follows from this, that in many cases, we cannot come to positive conclusions, without bringing together a greater number of specimens. This is likewise the case with states of fossilisation, in which points and tubercles are replaced by depressions,‡ long points by small drops, &c.§ One of the most remarkable modifications is that in which the external layers of a shell are always preserved in the rock, while the internal fibrous layers almost always disappear.‖ We may thus easily mistake the impression of the internal parts destroyed, for bodies altogether different¶ from the first.

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* Almost all the fossils of the superior Oxford stage in the vicinity of La Rochelle are in this condition.

† This is seen in the *Productus.*

‡ In the cretaceous fossils of Maus (Sarthe), alteration is frequent.

"A third cause of error, against which it is necessary to guard, is the state of preservation in which shells were before they became fossil. Every one may perceive, from examining the edges, that shells separated from their animal, are exposed to numerous causes of destruction. The least that can happen is, that they become rubbed, being rolled along by the motion of the water. Supposing that the same things took place before our epoch that occur in the present time, we must believe that shells exposed on shores to the incessant action of the waves, would necessarily become rubbed. We find, in point of fact, many beds in which the shells are rolled ;** and as they may render striated shells smooth, attenuate or change all the characters, it must be taken into account in modifications of this kind."

The deformation of fossil shells is likewise an important fact, and may often cause errors in the specific determinations.

"Although these deformations are of different importance, and altogether distinct according to the classes to which they belong, I must mention, in a few words, some of these general characters.

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* This is seen in *Cardium.*

† The *Petunculus* especially, and *Arca*, exhibit this character.

‡ I have seen this particularly in *Cardium productum* brought from Uchaux (Vaucluse).

§ This modification is common in the same species.

‖ This takes place in the *Hippurites* and *Radiolites.*

¶ Witness M. Defranca's genus *Iodamia.*

** This is seen in the inferior sandstones of the Turonian stage at Maus (Sarthe), in the Coral-rag of St Mihiel (Meuse) at Tonnerre (Yonne), &c.

"Shells are by no means deposited in the earth's strata, as certain individuals have supposed, according to their specific weight; they are found absolutely in the same conditions, according to which they are at present deposited in the sea on the shores, or, as we find them in modern deposits recently left by the sea.* Bivalve shells, for example, are in their normal position, that is to say, placed vertically, the side of the tubes upwards, the mouth downwards, in the argillaceous or calcareous beds of an infinite number of places belonging to all the different epochs.† They have been carried along by the currents, and deposited under the waters in horizontal banks,‡ or else heaped up on the shores by the waves.§ In the first mentioned case, the bivalves are in their place, as I have mentioned; the gasteropods have the mouth downwards. In the second case, the shells are deposited by chance, according to their forms; the flattest will be horizontally on the side, as the ammonites and the bivalves, and, finally, each will be found in the position most favourable to the equilibrium of the whole; but the gasteropods will be found with the mouth sometimes upwards, at other times downwards. In the third mentioned case, the shells still preserve in some degree the position relatively to their form, and the equilibrium of the whole; at the same time, as they are not deposited by a slow action, but by a sudden impulsion, they are found in all positions, without following any certain rule. It may be easily understood how we may determine, by means of these data, what has been the mode of deposition of the fossils contained in any kind of strata.

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* Those of the bay of Aiguillon, in the confines of the departments of La Vendée and Charente-Inferieure.

† I have seen them so placed in the inferior lias of Semur (Côte-d'Or), in the inferior Oolite of Coulie (Sarthe), in the Kimmeridgean stage of Havre, in those of Chatebaillon (Charente-Inferieure), and in the Portlandian stage of St Jean D'Angely, in the same department, &c. &c., in the Turonian stage of Montagnes des Comes (Ande).

‡ At Bayeux and Montiers, in the lower Oolite; at Luc, in the large Oolite (Calvados), &c. &c.

§ In the localities of the Coral-rag, which I have already mentioned, at St Mihiel (Meuse), at Tonnerre (Yonne), &c.

"The shells thus deposited, and more or less covered by posterior deposits, have so remained, with their shell changed into different substances. They have passed into the state of impressions, or else they have reached the condition of counter impression. If, after their deposition, the beds in the state of paste have sunk in their horizontal position, in consequence of the pressure of the whole ;[*] if they have been dislocated before this pressure, and a sinking or oblique slipping of the molecules, in relation to their first horizontal deposition, has taken place, it will be perceived that all the bodies found in these strata must have been subjected to the same pressure, horizontal or oblique, and will then become deformed in consequence of their relative position.

"Horizontal pressure, for example, produces a flattening of the shells in the direction of their compression. Accordingly, the nauliti, ammonites, in all the parts which were convex, are more or less flattened, and often become as thin as a sheet of paper.[†] Bivalves placed on the side lose half of their thickness, or become quite flat, and without convexity.[‡] We may likewise observe this simple compression in shells naturally compressed; but when it takes place in conical shells it may be conceived to change the specific characters altogether.[§]

"If deformation, in the direction of the compression of shells, may change their shape, this deformation will be much more considerable when it is exerted in the direction of their length. This takes place principally when the gasteropodes and acephales have preserved their natural position. Indeed, conical shells will become entirely flat,[‖] or their spire will change altogether from a spiral angle, and from being elevated, will become depressed, or even horizontal.* Accordingly, we must not take into account the spiral angle of the shells of the gasteropods in the state of counter-impressions deposited in calcareous and argillaceous beds, until we have compared them with a great number of individuals not deformed.

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[*] This has taken place in all the formations.

[†] This is seen in many ammonites of the foliated lias.

[‡] The possidonies of the lias present this depression.

[§] This deformation takes place in the Trochi and Pleurotomariæ.

[‖] The Patella and Orbicula.

"In regard to the acephals, deformation is one of the great causes of error. Such a shell being naturally oblong, when shortened on itself by vertical pressure, may become of greater breadth than height,† and undergo such a change in appearance as to be readily transferred from one genus to another. When, on the contrary, this pressure is exerted in the transverse direction of a shell, that is to say, from the hooks on the edge of the paleum, such a species, though at first round, may become oblong or even elongated,‡ undergoing a complete modification.

"The deformation produced by an oblique pressure, regard being had to the compression, to the length or the breadth of the shells, is more easy to determine in certain cases; but it is, on the contrary, the most difficult of all to establish in certain others. Oblique pressure has produced in the cephalopods, and in bellerophon, those elliptical spires which have given rise to separate genera.§ Some authors have likewise supposed that they have detected in it a distinctive specific character.‖ This same deformation likewise renders the spiral convolution elliptical in the gasteropods, by throwing the summit laterally, sometimes on one side, sometimes on the other.¶ If these deformations are easily understood by experienced eyes, such is by no means the case with the oblique deformations of bivalve shells. In these the pressure may not only render one valve more elevated than the other in symmetrical shells, and give them a greater or less resemblance to corbula* or thracia; but, besides, when it takes place in a vertical direction, passing between the two valves, and which it inclines more or less to the side of the labrum, this oblique deformation may modify the apical angle of a bivalve, and change its form to such a degree, without making it cease to be symmetrical,† that it may become very difficult to distinguish true species from deformations of this nature, which are, however, very common in shells which have preserved their normal position in the midst of argillaceous strata !‡ Not only, therefore, is it necessary frequently to disregard form, but also, in order to distinguish true species from accidental deformations, it is requisite to commence by seeking other exterior characters, and to compare, in this point of view, all the specimens that have been collected in the same bed and in the same place; for, in that case, the change of place and of strata must be allowed to have some influence in determining the limits of a fossil species.

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* I have noticed this deformation in many kinds of Trochus and Pleurotomaria.

† I possess the same species in all these deformations, which shall be figured at the head of each class. They are the *Cardium hillanum* and a *Panopæa* from La Malle (var).

‡ We find these deformations principally in the strata near mountains, as at Grasse (var.), at Castellane (Lower Alps), in the Corbières (Ande), and in a multitude of other places where the strata have been dislocated.

§ The genus *Ellipsolites* of Montford, originally adopted, since rejected by Sowerby.

‖ The *Bellerophon obliquus* of MM. Potiez and Michaud, is only a deformation of this kind in *B. Munsterii.*

¶ This is exemplified in some Pleurotomariæ in my possession.

"Referring to what I have already stated respecting the difficulties attending the positive determination of fossil species, I would say that these difficulties are so much greater, the more ancient the fauna that we investigate. In fact, the lower a bed lies, the more must the shells it contains have been exposed to dislocations, pressures, and the modifications to fossil forms. If, for example, the determination of species of extreme difficulty in the transition formations, when it is undertaken conscientiously; if it is still so in the cretaceous formations, after we enter upon the tertiary formations, such as those of the Parisian basin, it ceases altogether, and the determination of the fossil shells of this epoch enters into the category of that of living shells. It is only necessary, for the most part, to ake into account the natural variations which I have spoken f in treating of other shells."—(*Bibliothèque Universelle de enève*, No. xxii., p. 123.)

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* This deformation is very frequent.

† The *Pholadomya* are often found in this state, which has caused the species to be multiplied without end.

‡ They are found in a great number of places in France.

On the Geology of the German Tyrol and the origin of Dolomite. By Professor FAVRE of Geneva. Communicated by the Author.*

Desirous of extending the field of my geological researches, which for some time had been almost confined to the Alps of Savoy, I eagerly availed myself of a proposal of Professor Studer of Berne, to accompany him in an excursion to the Alps of the Tyrol. I was fortunate in undertaking this journey along with this *savant*, who is as amiable as he is distinguished. The route he had marked out enabled us to examine the different formations which constitute the surface of the Tyrol and Salzbourg.

I have no intention of describing them all; after a rapid glance at the topography and ancient formations of this country, I shall limit myself to a description of the pass of Heiligen-Blut-Tauern, and some considerations on the origin of dolomite.

The Tyrol, whose mountains form the eastern prolongation of the Alps of Switzerland, presents some differences from the latter, even in a picturesque point of view. The *large* mountains are less elevated and less numerous. The glaciers are not so large, they do not descend so far into the valley, which indicates that the snow fields of the upper regions are less extensive.

The general character of the mountains of the Tyrol is that they stand in a line following three great parallel chains, running very nearly from west to east.

The chain of crystalline rocks is situate in the centre; it is placed between Innsbruck and Klausen, or rather it is bounded by the upper part of the valley of Salza, and by that of the valley of the Drave, so wild in its character.

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* Read to the Societe de Physique et d'Histoire Naturelle de Genève, on 1st February 1849.

The two exterior chains are formed by dolomites and limestones, presenting an arid aspect. These mountains are so white, that their rocks are frequently confounded with the snows which occupy the most elevated summits.

The rocks which form these three chains are considerably varied in their nature, in the modifications to which they have been subjected, and in their age.

In the extensive depressions of the ground, which separate nearly all the exterior chains from the central one, we find many sedimentary formations.

Volcanic actions have here and there pierced the surface of this district, which has been so subject to geological accidents, and have brought various porphyritic rocks to the surface. They have thus complicated the structure and the nature of the ground.

The central chain appears to reach its maximum of elevation at Weiss-Kogl (11,840 feet*), near the glaciers of Œtz and Gross Glockner, which are 11,662 feet above the level of the sea. This sharp peak bears a great resemblance to the numerous aiguilles situate to the south of the valley of Chamounix. The Venediger-Spitz, at the bottom of the Pinzgau and the Wild Spitz, likewise among the glaciers of Œtz, almost rival the preceding mountains in height.

In the secondary chain to the north, the most elevated summit appears to be the Dachstein (9234 feet†) in the Salzkammergut; in respect to elevation, then come the Ewiger-Schneeberg, the Steinernes-Meer, &c.; while, in the southern chain, the rocks of Marmollata, attaining a height of 10,400 feet, appear to exceed the other parts of the chain.

The crystalline rocks of the central chain are formed of true granite, the component parts differing in size. Often, without becoming exactly gneiss, this rock assumes an appearance which Saussure has well named *veined granite.*‡ The granitic rocks are highly developed at the foot of the Zillerthal, between St Jacob and Pfunders. This granitic axis is intersected at Mittelwald by the road from Sterzing to Brixen.

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* These measurements, taken from G. Mayr's map (Munich, 1846), are in French feet.

† According to M. Simony, 9493 Viennese feet (Memoires de la Société des amis des Sciences de Vienne, t. i., p. 317, 1847).

‡ Voyage dans les Alps, § 163.

The central chain likewise contains many other rocks, such as true gneiss with white or black mica, as in the Zemm-Thal, and in the bottom of the valley of Gastein, where they are associated with hypersthenic syenite, at Isselberg, near Lienz, and at Dollach, in Carinthia.

It appears to me that true protogine is wanting in the Alps of the Tyrol.

The metamorphic rocks and stratified formations, not fossiliferous, constitute an ill-defined group, because, in the lower part, they pass into crystalline rocks, and, in their upper part, into sedimentary fossiliferous rocks. They often themselves present characters of crystallisation.

We have carefully studied these complicated rocks in the long pass of Pfitsch-Joch.

The most widely distributed, most common, and most important rock of this group, is an argillo-talcose slate, whose characters are very variable. The Tyrolese geologists distinguished it by the name of argillaceous mica slate (Thon glimmerschiefer). Near Zell, a mine of pyrites and auriferous mispickel has been opened in this rock, which, according to M. Burat, produces annually 35 marks of gold from 50,000 quintals of ore. Native gold is sometimes found likewise. These ores have been probably formed at the same time with the veins of quartz which traverse the rock.

This argillo-talcose slate, enriched with an immense quantity of garnets, occupies the summit of the pass of Pfitsch-Joch (6741 feet), and is approached by varieties of gneiss, slaty serpentine, and clay slate.

It is well to observe the great analogy which exists between these rocks and those of the Canton of Valais. Their resemblance is such that we ought not to despair of finding fossils in them. In fact, we know that, in the latter country, MM. de Charpentier and Lardy have found belemnites in these rocks associated with the garnets.*

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* Lardy's Essay on the geognostic constitution of St Gothard. (Memoires de

These slates, so varied in character, are evidently mineral masses which have been more or less altered by different agents, among which we must rank an elevated temperature. The proof of this action of heat is the following. These slates, as we have mentioned, are more or less crystalline; now, it is in the most crystalline part that we find the greatest number of those masses of saccaroidal or granular limestone, which, according to Hall's experiments, indicate a powerful action of heat. These masses are disposed in the argillo-talcose slate formations in large lenticular masses and beds, parallel to the stratification.* In general, these limestones are found at the lower part of the stratified rocks, as, for example, at the northern extremity of Zemm-Thal, where the saccaroidal limestone is slaty in contact with the gneiss, but compact, sonorous, and breaking like glass, at a distance of ten or fifteen yards.

Other masses of saccaroidal limestone may be observed in the Ziller-Thal. We have found the continuation of them, first, to the west of the Brenner road, where the sections, published in the Comptes rendus of the Montanistitche Verein (1843), indicate that their beds have a non-conformable stratification with the mica slate; and, secondly, twenty leagues more to the east of the same chain, at the picturesque pass called *Klam*, at the entrance of the valley of Gastein, near Lend. This section, narrow and deep, through which, notwithstanding, the waters of this valley are discharged, has not always existed, for the valley of Gastein presents all the characters of an ancient lake.

The rocks which form the Klam are white or greyish saccaroidal limestones, more or less charged with mica (talc?). This limestone presents three very distinct appearances; 1*st*, Homogeneous or compact, although saccaroidal; 2*d*, Slaty and slightly grooved, the surface of the slates being faintly undulated; 3*d*, *Bacillary*, that is to say, formed

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la Société helvetique des Sciences Natur., tome I, p. 241, 1833.) Studer. (Memoires de la Société geologique de France, 2me Serie, t. i., p. 308.)

* The saccaroidal limestones of Meran, which are so skilfully employed in the workshops of M. Schwanthaler, at Munich, are probably found in the same geological position.

of prismatic distinct concretions, and fitting to each other, as may be seen in small billets of wood split in the direction of the fibres; only these small concretions leave no empty spaces, and sometimes become so slender that the rock resembles hard asbestus. This state is the result of the maximum of development in the circumstances which produced the slaty structure;—it is that structure carried to the extreme. Certain crystalline slates present some resemblance in their structure to this limestone. Only, these rocks being formed of many mineral substances, the structure is not so regular. We notice in it nodules, and imperfect crystals of quartz or felspar, which are enclosed in cavities more or less deep, lined by one of the substances of the slate in leaflets (mica, talc, or steatite). This substance appears to have been subjected to friction, for it is marked with small striæ on the surface. The greater part of the rocks known under the name of satin-slates present this same character.

In a question so complicated as that of the origin of the slaty structure, we ought not to have recourse to a single action in order to explain it. Accordingly, it is admitted that, in certain cases, this structure has been produced by the fusion of rocks, which have flowed downwards;* and that, in other circumstances, it represents the remains of stratification. Lastly, we may perceive further, that this phenomenon owes its origin to abrasions and frictions,† which have taken place before the complete solidification of the rock. This latter mode of formation appears to me evidently demonstrated by the resemblance of the crystalline slates to the rocks of Klam. The different forms of these limestones, taken in connection with the observation made at the entrance of the Zemm-Thal, appear to us to indicate that the slaty structure, in the case of which we speak, is the result of frictions and *etirements*. To this it must be added, that the saccaroidal limestones of the Tyrol are not eruptive limestones, because they are placed in the form of bands or beds lying parallel to the central chain.*

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* Naumann. (Neues Jahrbuch für Minera., 1847, p. 297. Archives de la Bibl. Univ., t. vii., p. 322; 1848.)

† Studer. (Memoir formerly referred to). Fournet (Annales de la Société d'Agriculture de Lyon, t. iv.; 1841 et 1846.)

Lastly, a locality where we have seen the saccaroidal limestone developed to a great extent, is the pass of Heiligen-Blut-Tauern.

The pass of Pfitsch-Joch having brought us to the southern acclivity of the Alps, we selected the pass of Heiligen-Blut for our return to the north, as being elevated (8,051 feet), and very near to Gross-Glockner, so that we hoped to find in it as much scientific interest as picturesque beauty. Our expectations were in no degree disappointed.

The village of Heiligen-Blut is in a charming situation. Its little church, picturesquely placed on a hill, overlooks a valley covered with beautiful trees and cottages. The somewhat steep walls of this valley form a frame in which rises the sharp aiguille of the Gross-Glockner, of which the Tyrolese and Carinthians never speak without admiration.

Hacquet was one of the first who wrote about this mountain. He estimated its height at 10,000 feet, in his mineralogical and botanical journey from Mont-Terglou in Carnia to Gross-Glockner, published in 1784, a little before Saussure had ascended to the most elevated point of Europe. From that period the environs of Gross-Glockner have not failed to be frequently visited, and they always excite a just admiration.

On leaving Lienz, in order to reach the pass of Heiligen-Blut by ascending the valley of the Möll in Carinthia, we find crystalline rocks on the northern bank of the Drave. Indeed, while passing Isselberg, we walk over rocks of amphibole, embellished with garnets, forming masses of greater or smaller size in the mica slates (direction, N. 80, to 85 E.) In the upper part of the valley of the Möll (between Dollach and Putschal) we fall in with frequent associations of saccaroidal limestone, cipolin, serpentine, serpentineous porphyries, and green slates.

In traversing the pass of Heiligen-Blut-Tauern, we fancy that we would walk over crystalline slates or granites;

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* Fournet. (Comptes Rendus of the Academy of Sciences, p. 406; 1844.)

but, to our great surprise, we scarcely met with anything else than limestones. Above Heiligen-Blut we traverse green slates and slaty cipolins with white mica, the strata dipping nearly to the south. We soon reach argillo-talcose slates containing garnets, accompanied with dolomite and saccaroidal limestone. After advancing about three hours, we find ourselves on a kind of plateau, terminating in a circus, the beds of which are horizontal. When we reach the highest part of this circus, formed by argillo-talcose garnetiferous slates, the green slates likewise containing garnets and quartzite, rather in veins than in beds, we have gained the summit of the pass.

From the commencement of the descent, we find large beds of saccaroidal limestone more or less micaceous. The green, or argillo-talcose slates, alternate with limestones, and form, with some dolomites and *cargneules*, all the northern acclivity of the pass as far as Tauernhaus, where serpentine occurs. The horizontality of the beds at the summit of the pass appears to continue as far as the junction of the Seidlwinkel-Thal and Rauris-Thal, and beyond, the strata dip to the north. To recapitulate: We have seen in this pass, 1*st*, That the central chain was formed, not of crystalline rocks, but in a great part of limestones and slates, which are evidently sedimentary rocks more or less altered; 2*d*, That these sedimentary formations form a saddle or arch; for, on the side of Heiligen-Blut, they dip to the south; on the summit of the pass they are horizontal, and, on the northern acclivity, they dip to the north. It is probable that this arch covers the prolongation of the granitic rocks of the bottom of the valley of Gastein, and that the formations composing the actual arch are only a very small part of that which formerly had a tendency to become formed when the upraising of the central chain heaved up the great masses which form the secondary chains of the Tyrol.

The argillo-talcose slates and saccaroidal limestones alternate in a general way with the green slates, which are more or less serpentineous, chloriteous, or talcose, and which are nothing more than modifications of the first of these rocks; as a proof of this, we may refer to the numerous instances in which these two rocks pass into one another. It will be further remarked, that the circumstances which have developed the garnets in the argillo-talcose slates have produced the same effects in the green slates, for in these garnets are abundant. The green slates are developed in the neighbourhood of the serpentines and amphibolic rocks; and we may regard both of these rocks as being the maximum of alteration in this great formation of slates presenting such varied characters.

On ascending Pfitsch-Joch from Zell, we find porphyroidal amphibolites, other varieties in which the amphibole is radiated, argillo-talcose slates, green garnetiferous slates, and dolomites; these rocks constitute the mountain Greiner (8800 feet), celebrated for its minerals. It is near the granitic chain of the bottom of the Ziller-Thal.

The green slates, containing a variable quantity of epidote, form a great part of the mountains of the valley of Gastein, particularly between this valley and Rauris.

The serpentine, as I have mentioned, is disposed in masses throughout the formations of which we are treating. In general, it is placed in beds, or in fragments of beds, parallel to the central chain. This arrangement indicates that this rock is not one of eruption, but that it is the produce of strongly developed metamorphic action. I have found this rock at Pfitsch-Joch with a species of euphotide, near Dollach in Carinthia, at Matrey between Innsbruck and the Brenner, where it is mined along with ophicalce; it is everywhere surrounded with large masses of green slate.

The non-fossiliferous stratified formations are, therefore, composed of argillo-talcose slates, green slates, limestones, dolomites, amphibolites, serpentines, and ophicalces, the varieties of which are infinitely numerous. The principal rock is the first of those we have indicated; the others occur in the form of beds, or fragments of beds, parallel to the central chain.

The presence of the Silurian formation in the Tyrol has been determined a few years since. The following are our observations on this subject.

When traversing the valley of Gastein, from south to north, and receding from the granitic and central chain, we perceive that the mountains which flank the valley are composed of the formations we have described; we then traverse the Klam, formed of saccaroidal limestone, and arrive at the small town of Lend. If we go from that place towards the iron mines of Dienten, we pass over the edge of strata which run southwards, and consequently rest on the central chain. These strata are formed of grey argillo-talcose slates and green slates, alternating with each other, and identical in appearance with the most abundant beds of the southern Valais; near Dienten (above and below the village), these formations contain beds of carbonated iron, which dip at 30° to the north. These beds, which are mined, closely resemble the mines of spathic iron in Dauphiny. They contain crystals of spathic iron, rhombohedric, lenticular, &c., which often rest on crystals of quartz. The important fact is, that, in the interior of these iron mines, a thin bed of graphite is found, very nearly pure, containing fossils referrible to the Silurian epoch. Those in the collection of the School of Mines of Vienna bear the following names:—*Cardium gracile*, Munst., *Cardiola interrupta*, Brod., *Orthoceras styloideum*, Bar., *O. gregarium*, Munst.*

Although we examined the geological position of these mines, we cannot give any positive opinion respecting their age, the fossils in our possession not being sufficiently numerous. Without venturing on any exact determination, we may state that we have been struck with the striking resemblance which exists between the orthoceratites of Dienten, which are called Silurian, and those of St Cassian, which are evidently from the muschelkalk. It would, indeed, be less extraordinary to find at Dienten fossils of this latter formation than those of the Silurian epoch.

However this may be, the discovery of these fossils, which was made only a few years ago, promises to be of great importance in the geology of the Alps; for the rocks of Dienten are similar to those of the Valais and Dauphiny.

With regard to the true coal formation, its presence is much more certain than that of the Silurian formation. It is developed at Stangalp, to the west of Gmund, and to the north of Villach, as is mentioned by M. Morlot, in his explanation of the Geological Map of the Alps, and as is proved by specimens in the Museums of Vienna and Linz; the latter come from Rosaninalpe to the south of the Radstadter-Tauern.

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* Morlot. *Erläuterungen.* Explanation of the Geological Map of the N. E. portion of the Alps, p. 131. 1847.

Ascending in the geological scale of formations, we find the red sandstones, the trias and dolomite, formations which are associated with and traversed by the quartziferous and pyroxenic porphyries. The observations made by us on these varied rocks are the following:

Turning eastward from Klausen, in the valley of the Eisack, by the Grodner-Thal, as far as St Cassian, we pass over a transverse section of the most interesting nature. But, in order to obtain a general view of the whole, it is necessary to make several detours, such as those of Castelruth, the Seisser-Alp, and Langkogl. The Eisack cuts through different porphyritic rocks, which seem to form the base of the sedimentary formations; then, ascending to reach the Grodner-Thal, we walk over argillo-micaceous and talcose slates; and near the village of St Peters, we arrive at masses of red quartziferous porphyry, which, considered on a large scale, appear to form a bed rising towards the west, but to which two systems of fissures, which cross each other, have given an irregular prismatic structure. This porphyry is usually altered on the surface; it is covered by the red sandstone, the beds of which have a stratification conformable with the porphyry. M. de Buch has long since shewn that the porphyry was the mother-rock of the red sandstone.

Above the red sandstone, which is highly developed in the neighbourhood of Castelruth, are found different beds which are referred to the trias system; but it is not always easy to seize exactly their geological position, for the vicinity of the quartziferous porphyry, especially that of the pyroxenic porphyry and the immense quantity of pyroxenic tufa, which, after alternating with these, have altered them, and introduced among them the elements of the plutonic rocks, which render their characters very variable. Above the red sandstone we generally find the muschelkalk, the rock of which is a pretty compact limestone; sometimes, however, it occurs in reniform masses, nests, or crusts, of a greenish substance, which resembles decomposed pyroxene, and in which we observe fragments of pyroxene.

It is evidently to the trias system, and more especially to the muschelkalk, that we must refer the celebrated fossil beds of St Cassian. These beds have not the least connexion, in their geological position, with the neocomian formation, in which some geologists have endeavoured to classify them. The locality richest in fossils is Steurs, situate two hours to the south of St Cassian, on the summit of the hills, clothed with pasture and woods, which separate the valley of the Badia from that of Livinalongo (likewise called Buchenstein or Fodom), near the sources of the Gader. The position of the beds which contain these fossils, and of those corresponding with them, becomes evident when we arrive at St Cassian by the Grodner-Thal, in passing the Col de Colfosco. These beds are situate, undoubtedly, below the dolomitic masses. They do not always contain the fossils which abound at St Cassian, and the variability of their characters, caused by the greater or less abundance of the elements, arising from the submarine eruptions which have taken place in their neighbourhood, are the principal obstacles to their being recognised over a large extent. However, M. Emmerich has found some fossils of St Cassian above St Michel, and in the ravine at Pufl.

If the hills of Steurs, where the St Cassian fossils are found, are not covered by dolomite, it must be ascribed to an immense denudation which has carried off the latter rock.

These fossiliferous beds are remarkable for the curious association of othoceratites with ammonites, and the development of a mass of small shells, which, for the most part, appear to be young. We have ourselves collected these fossils, and their position altogether excludes the idea of a *remaniement dans les terrains.*

On examining the position of these fossil beds, we perceive that, in the bottom of the valley of St Cassian, the pyroxenic conglomerate occurs covered by varied sandstones, and by a calcareous conglomerate, containing numerous nests of a green matter resembling, as we have said, docomposed pyroxene, and enclosing fragments of that mineral. These sandstones and conglomerates are covered by the muschelkalk, which is a more or less marly limestone.* We perceive that the base of Kreutzkogl, near St Cassian, presents nearly the same section, and that this mountain, which is dolomitic, rests on the muschelkalk.

The superposition of the dolomites on the muschelkalk is likewise seen in the north part of the base of Langkogl and at the Col de Colfosco, where we find a greenish sandstone partly formed out of the elements of the pyroxenic rocks. This sandstone appears to be contemporaneous with the muschelkalk, and contains small fossils which cannot be determined. (Astartes ?)

On ascending above the baths of Razes, we perceive on the muschelkalk the pyroxenic tufas which form the surface of the Seisser-Alp. This name is given to a great plateau bounded by the Schlerns, the Palat-Spitz, the Blattkogl, the Langkogl, and the Grodner-Thal. It is clothed with pasturage and slightly inclined towards this valley. This pyroxenic tufa, which is a kind of sand, more or less coarse, containing numerous small crystals of pyroxene, is evidently dependent on the pyroxenic porphyry seen at Pufl, St Christine, &c. The tufa is perfectly stratified; it is formed by alternations of substances, loose or of greater consistency. We here observe rolled pebbles of all sizes, which have evidently been tossed about by the waters, and are a submarine dejection. What farther proves this are the numerous alternations of this tufa with the limestones and dolomites which may be observed in a remarkable escarpment situate near the chalets of Molignon, not far from the pass which leads from the Seisser-Alp to Campidello in the valley of Fassa.

In this natural section which overlooks the chalets, we have counted more than ten beds of dolomitic limestone, separated by as many strata of pyroxenic tufa. Has there not evidently been a contemporaneous formation between the plutonic and sedimentary rock?

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* The general view which I give here is sufficient for the end I propose, and I refer for the details to the *Coup d'œil sur la Geologie du Tyrol meridional*, by M. Emmerich, published in the *Alpes de l'Allemagne*, by Schaubach, tom. iv. 1846.

On a geological inspection of the country, it seems as if we had left the Alps, and have been suddenly transported into the region of extinct volcanoes in the centre of Sicily. Analogous alternations are seen above Colfosco.

The ravines which intersect the great plateau of the Seisser-Alp, disclose to our view spilites with beautiful zeolites, and the surface of this plateau is strewed with masses of true dolomites containing oysters (?), beautiful remains of polypi and stalks of encrinites.* The position of these dolomites indicates that they are the remains of the denudation which has formed the plain of the Seisser-Alp.

These pyroxenous porphyries, and the rocks depending on them, appear to be completely wanting to the north of the central chain.

The dolomite of the Tyrol, like that of Switzerland, occurs in two positions; 1*st*, It is found in the mass accompanying rocks more or less crystalline, such as the argillo-talcose slates of which we have spoken; in general this dolomite is in the neighbourhood of rocks really plutonic. 2*d*, It constitutes great masses, and forms almost of itself the two lateral chains of the Austrian Alps. These mountains are distinctly stratified. It is sufficient to have seen the great mass of the Schlerns, from Castelruth or the cemetery of Layen, or still better, the more imposing mass of Kreuzkogl from Colfosco, to be convinced that their stratification is very nearly horizontal. However, in the chain of the north, as in that of the south, the beds are more or less turned up against the central chain, a circumstance which confirms what we have said respecting the correspondence of these two chains.

I know not at what period in the history of the globe the eruptions of melaphyres have ceased to take place. Some authors think that they terminated during the tertiary epoch, which may be the case. With regard to the epoch at which they commenced, M. de Buch says, that the elevation of pyroxenic porphyry is posterior to the secondary formations, because it pierces the different beds of it. (*Annales de Chimie et de Physique*, 1823, xiii., 293). I think that it is necessary to make a distinction here. The eruptions of these melaphyres have perhaps continued a very considerable time, and even though a portion of these rocks has reached the surface after the deposition of the secondary formations, according to the learned geologist of Berlin, it appears to me certain that these eruptions have commenced at the period of the muschelkalk. Indeed the observations which I have already referred to, and which I recapitulate here, demonstrate that there were eruptions of melaphyre contemporary with the muschelkalk, and anterior to the formation of dolomites.

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* M. Morlot points out corals in the dolomite of the Seisser-Alp; M. Bertrand Geslin has found other fossils in the volcanic tufa of this locality (*Bulletin de la Société Geologique de France*, First Series, iv., p. 8.)

As a proof, I have indicated, 1*st*, The superposition of the dolomites on the pyroxenic formation, a fact clearly determined by the general examination of the country at Grodner-Thal, and in the neigbourhood of St Cassian. This superposition is seen at the ravine of Pufl, at Palat-Spitz, at Langkogl, and in the sections given by M. de Buch (*Annales de Chimie et de Physique*, 1823, xxiii.) 2*d*, The pyroxenic rocks, as I have stated, have furnished the materials of certain beds of muschelkalk inferior to the dolomites. 3*d*, It may well be that the pyroxenic rocks have been erupted before the formation of dolomites, since their stratified tufa alternates with dolomitic beds, situate at the inferior portion of the great masses of dolomite.

This is a point which I wished to establish in a positive manner, for that was necessary in order to understand the origin of the dolomite.

The formation of dolomite is an important question, and one which has given rise to so much writing and discussion, that it is difficult not to touch upon the notions which have been advanced on the subject; the more so, as certain authors have treated of it in a manner so general and vague, that they seem to have wished to include in their system the greater part of former theories, as well as the germ of all future theories. Notwithstanding this, I have endeavoured, by special reference to certain facts, to bring them under one point of view, which appears to me to present some novelty. Moreover, the experiment made by my colleague, M. Professor Marignac, and which throws great light on the formation of dolomite, is entirely new.

In spite of the ingenious theories advanced respecting the origin of this rock, many doubts still remain in science regarding it, for chemical experiments have not always confirmed geological theories.

I here speak only of the dolomites which belong to the second of the geological positions I have indicated—that is, the dolomites of the great secondary chain of the Tyrol; and I think that they are not a metamorphic rock, in the sense usually given to that word—that is to say, that the rocks of these chains have not been altered since their formation. I am of opinion that these mountains have been composed, from their origin, of a double carbonate of lime and magnesia—that is to say, that the formations which constitute them have been deposited in the state of dolomites at the bottom of seas, and are not limestones altered by magnesian vapours.

I rest this affirmation on the beautiful researches of M. Haidinger, which have been brought forward by M. Morlot,* and on the experiment of Professor Marignac.

We must not overlook an important fact, well known for a long period, which has even given rise to theories as to the formation of dolomite, that there exists a certain connection between the dolomitic chains and the pyroxenic rocks, which indicates that it is the latter which have furnished the magnesia, in whole or in part, to the dolomite.

M. Haidinger has succeeded in making dolomite, by heating to 200°, and under a pressure of 15 atmospheres, a mixture of sulphate of magnesia and carbonate of lime—that is to say, it is necessary, in order to form dolomite, that there should be, 1*st*, sulphate of magnesia and carbonate of lime; *2d*, a temperature of about 200°; *3d*, a pressure of 15 atmospheres. Now, I believe that these circumstances, by no means complicated, may have been met with in the localities actually occupied by the dolomitic chains of the Tyrol. *1st*, It is evident that in the sea, where the formations composing these chains were deposited, lime existed. No one has ever doubted this fact; besides, the encrinites, oysters (?), and the beautiful corals of the Seisser-Alp, are sufficient to prove it. With regard to the sulphate of magnesia, we know that it exists in the waters of the sea; but I am of opinion that a more considerable quantity than ordinary existed in this sea; and this is my reason—the pyroxenic tufa, as we have said, is the produce of submarine eruptions, and consequently gases, and, among others, sulphurous acid, which always accompanies volcanic eruptions in great abundance, are more or less dissolved in the water; they have formed different salts with the substances which were present. The rocks which were erupted, being very rich in magnesia, sulphate of magnesia must have been formed soluble in twenty parts of cold water, and in much less of boiling water, according to Berzelius. This salt is met with in the neighbourhood of existing volcanoes, and readily passes into the state of sulphate of magnesia by the action of the air (Thenard). Thus the presence of a notable quantity of sulphate of magnesia in this sea is placed beyond doubt.

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* Comptes Rendus de l'Acad. des Sciences de Paris, 6th March 1848, Archives de la Bib. Univ. de Genève, April 1848.

2d, A temperature, I have said, of 200° C. was required. Such a temperature assuredly existed at a certain depth in a sea where volcanic eruptions took place, and whose bottom was covered with a greater or less quantity of muddy and sandy substances.

3d. A pressure of 15 atmospheres. This condition is found to be exemplified in a sea whose depth is only from 150 to 200 yards. It is evident that the sea in which such immense masses as those forming the dolomitic chains of the Tyrol were deposited, was of a much greater depth.

We have thus all the conditions required for the formation of dolomite, and which must have been met with in nature, according to the ordinary course of things.

It may also be remarked, that hydrochloric acid is likewise disengaged in great abundance during volcanic eruptions; and from this *chloride* of magnesium must be formed, and added to that which already exists naturally in marine waters in greater quantity than the sulphate.

This remark has suggested the idea to Professor Marignac, to endeavour to ascertain whether the chloride of magnesium, brought into contact with carbonate of lime, might form dolomite in certain circumstances. For this purpose, he has made an experiment analogous to that of M. Haidinger—that is, he has placed a certain quantity of carbonate of lime, prepared chemically, and a dissolution of chloride of magnesium in excess, in a tube of thick glass. The tube was closed after the expulsion of air, and subjected to a temperature of 200° C.

An analysis of the produce was made on $0^{\text{gr.}}770$ of the matter taken from the tube. The magnesia was brought to the state of phosphate, and the following obtained:—

Carbonate of lime, ...	0·370
Phosphate of magnesia, . .	0·533

We find, then, as the result of the operation—

$$\text{Ca. C. } 48 \left\{ \begin{array}{l} \text{Ca. } 27 \\ \text{C. } 21 \end{array} \right.$$

$$\text{Mg C. } 52 \left\{ \begin{array}{l} \text{Mg. } 25 \\ \text{C. } 27 \end{array} \right.$$

which shews, that a double decomposition took place in the tube; that there is formed a double carbonate of lime and magnesia, and of chloride of calcium, which remained in dissolution. Not only has the decomposition been sufficiently complete to form dolomite, but, further, a double carbonate is formed, containing a quantity of magnesia exceeding that of true dolomite. This kind of rock is frequently met with in nature.* The chloride of magnesium may therefore form dolomite with the limestone, when it is subjected to the same conditions as the sulphate of magnesia. M. Marignac, having made the same experiment by exposing the tube only two hours to a temperature of 200°, found that the result was only a magnesian limestone but little charged with magnesia. Thus, then, the length of time during which combination may take place, is one of the numerous circumstances which have an influence on the formation of dolomite. By supposing it to be the only agent, we may be able to comprehend how it is that we find in nature, magnesian limestones, dolomites, and *surdolomitic* limestones.

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* In Dauphiny. Gueymard (*Bulletin de la Société Geologique de France,* 1^{re} Serie, xi., p. 438.)

After these different considerations, we have no doubt that hydrochloric acid, the different acids of sulphur, and in particular, sulphurous acid, which have accompanied the submarine dejections of the pyroxenic tufa, have acted upon this rock only in producing different salts of magnesia, which, under the double action of a pressure of about 15 atmospheres, and a temperature of 200°, have formed dolomite and magnesian limestone by means of the lime, whose presence, in seas, is attested by the corals and shells still found in dolomite.

It is necessary, however, to remark, that dolomite presents a particular character not found in rocks as at first formed, and which seems to indicate that it has undergone modifications since its formation. This character is given to it by the numerous cells or small cavities, and by the multitude of pores or empty places which it presents.

These cells, according to M. Elie de Beaumont,* are the result of the difference which exists between the atomic volume of the magnesia and that of the lime, and prove that the dolomite is an altered limestone, in which an atom of lime has been replaced by an atom of magnesia. M. Morlot has confirmed this view, by shewing that the empty spaces or cells of the dolomite exactly represent the difference of the volume of an atom of magnesia and that of an atom of lime.

This character, the cavernosity of dolomites, is therefore important; it must be taken into account. Now, the following is the way in which things may have proceeded in nature.

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* Bulletin de la Société Geol. de France, viii., p. 174; 1837.

It is not necessary to suppose that the beds which form the great dolomitic masses of the Tyrol were at first deposited in the state of limestone, and that they were then changed into dolomite at a period more or less remote from the time of their deposit; that is to say, after these beds had attained the enormous thickness which they now present. It is by no means probable that these beds of cellular dolomite were deposited in the state of dolomite, for the rock would be compact;* but we may conceive an intermediate between the two modes of formation in order to explain the origin of the Tyrolese dolomites, which are celluar throughout the whole of their enormous mass, and admit that as fast as the limestone was precipitated, in a form more or less pulverulent, it was changed into dolomite; and this kind of metamorphism of the limestone, which took place after its formation, well explains the cavernosity of the dolomites, and enables us to understand their stratification.

Saline substances may have been more abundant in ancient seas than in the present ones, without organic life being thereby destroyed; this is proved by an observation of M. de Verneuil who saw in the Crimea a species of cardium and other shells living in lakes where the saline substances were so abundant that they frequently crystallised in summer.† This is the reason why we find fossils in the dolomites of the Tyrol, although they are not very abundant, and the mode in which dolomite is formed, explains why the shell of these fossils is frequently dolomitic.‡

In these ancient seas, as in the present ones, shells and corals lived at a small depth below the surface of the water; there they secreted lime, and, it is probable, that the transformation into dolomite only took place when the precipitated lime reached a certain depth, that is to say, a certain pressure.

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* I may say, however, that compact dolomites are found in sedimentary formations of different ages, and, consequently, we may suppose that there is a certain class of these rocks which were at first deposited in the state of double carbonate of lime and magnesia.

† Verneuil (*Mem. de la Société Geolog. de France*, iii., p. 9).

‡ Collegno (*Mem. de la Soc. Geolog. de France*, x., p. 310).

According to this mode of viewing the great phenomenon of the formation of the dolomites of the Tyrol, we perceive why these rocks approach, to a certain point, the eruptions of pyroxenic porphyries, without, however, being completely connected with them; in fact, the sea, where these volcanic eruptions took place, and in which the circumstances fitted for forming dolomite were united, extended to a great distance. Yet, the dolomitic sediments must have been made with greater activity in the neighbourhood of the centres of eruption. We may thus explain why the secondary chain, placed on the northern declivity of the central chain, is likewise dolomitic, although there was no pyroxenic eruption in that place; for, at this remote epoch the central chain was not yet elevated, and the formations which at a later period were to form the secondary chains of the Tyrol were deposited in the same sea.

From the decomposition of the sulphate of magnesia by the carbonate of lime, the sulphate of lime must have resulted, and this salt has been precipitated in a warm state; for a more elevated temperature than ordinary was a consequence of the submarine eruptions of which we have spoken. Now, Mr Forbes has remarked, that the sulphate of lime so precipitated was anhydrous.* Here then, we have the explanation of the formation of anhydrite, a substance which, according to the observations of M. de Charpentier, has furnished the gypsum of the Alps. We know that this latter rock is frequently met with in the Tyrol, near Vigo, at the Seisser-Alp (De Buch), and in the valleys of St Vigile and Untermeyer; besides, the gypsum being a soluble rock, it is probable that it is now found in fewer places than it was formed. However this may be, the presence of this rock indicates that sulphuric acid has not been a stranger to the formation of dolomite.

We have distinguished two species of dolomite; their geological position is found to be sufficiently explained by this theory. Some of them are regularly stratified, as in the mountains of the Tyrol; this is a regular sedimentary formation similar to that of limestone, although, perhaps more complicated. The other dolomites are crystalline, saccaroidal (at St Gothard, Pfilsch-Joch), in their position corresponding to that of the saccaroidal limestone; they have undergone a metamorphism similar to that of this rock; and, as M. Fournet says, in speaking of predazzite, we may assert that we ought not to see in saccaroidal dolomite an effect of magnesian cementation, but rather the simple fusion of a limestone already magnesiferous.*

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* Letter to M. Morlot (*Comptes Rendus*, already quoted).

We perceive, then, that this theory on the origin of dolomites does not rest on the vaporisation of the magnesia, an occurrence which is known neither in nature nor in laboratories, but is founded, 1*st*, On the consideration that the eruptions of melaphyres (magnesiferous rocks) are anterior to the formation of dolomites; 2*d*, That these eruptions were submarine; 3*d*, That they were accompanied with acid vapours which formed salts of magnesia, which, under the circumstances of pressure, and suitable temperature (somewhat high), have modified the composition of the newly-formed limestone. There is no fact in this theory which was not previously known, and which is not daily repeated, so to speak, both in nature and in laboratories.

"More to the west, in Switzerland and in Savoy," says M. de Buch, in the conclusion of his celebrated Memoir on the Dolomites of the Tyrol, "we observe none of those phenomena we have been discussing, and which, considered in their mutual connection, may throw some light on the formation of the high chain of the Alps."—(*Annales de Chimie*, xxiii., p. 407; 1823.)

This assertion appears to us to be too positive. Indeed, although we can see nowhere in Savoy great chains of mountains formed by dolomites so white and remarkable as those of the Tyrol, this rock is still abundant in the regularly stratified formations, and in particular in Chablais and Faucigny. Is the dolomite, in these localities, the result of submarine eruptions, analogous to those of the Tyrol? that is to say, did there exist in the sea which deposited this rock, a greater quantity of sulphate of magnesia and chloride of magnesium than in the present seas? Without being certain, this appears to us probable; for we likewise find in Savoy evident traces of submarine eruptions. These traces are furnished by the rock long known under the name of Taviglianaz sandstone,* in a locality in the chain of Diablerets, where it is much developed.

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* Annales de la Société d'Agriculture de Lyon, iv., p. 12.

This rock, which is widely diffused in Savoy, and in the Cantons of Vaud and Berne, has not the same composition as the pyroxenic rocks of the Tyrol; but it has a close relation, in its geological position, with the pyroxenic tufas of this country.

The sandstone of Taviglianaz, of which we can distinguish many varieties, is usually formed of white felspar in small crystallized fragments; blackish or greenish amphibole, in fragments of imperfect crystals, but in which we can determine an angle of 124°; of white or black mica in scales by no means abundant, and of quartz in small fragments, some of which are so rounded that they appear to have been rolled. Some specimens of these rocks effervesce with acids. This is not, therefore, a pyroxenic tufa; but rather, if we might venture to make this supposition, a syenitic tufa. This rock has a position analogous to pyroxenic tufa; in this sense, that it is stratified, and alternates with beds really formed by way of sediment, such as limestones more or less argillaceous. It is a rock of igneous origin, triturated and stratified by the waters. With regard to the age of its formation, it differs considerably from that of pyroxenic tufas, with which we are at present specially occupied; for it usually covers the nummulitic formations, and is itself covered by the flysch, or Alpine macigno.

Is this rock connected with the pyroxenic eruptions which have taken place, according to some authors, in the tertiary epoch? This is a point we cannot now decide. However this may be, it is not less true that the eruptions which have furnished the materials of the Taviglianaz sandstones, may have been accompanied with disengagements of sulphurous acid and hydrochloric acid, and exercised an influence on the formation of the dolomites of Savoy analogous to that which the pyroxenic eruptions of the Tyrol have exerted on the origin of the dolomite of that country.

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* As early as 1834, M. Studer, in his work on the Western Alps, compared the sandstone of Taviglianaz to a volcanic tufa.

On the Colour of Water. By Professor BUNSEN.

The hot springs which occur in many parts of Iceland, and are especially remarkable at Reykir, are, says that excellent observer Bunsen, characterised by extreme beauty. In the depths of the clear unruffled blue waters of these basins, from which rises a light vapour, the dark outlines of what once formed the mouth of a Geyser may be faintly traced amid the fantastic forms of the white stalactic walls. Nowhere can the beautiful greenish-blue tint of water be seen in greater purity than in these springs.

A few remarks on the causes from which they are derived will hardly be superfluous.

Chemically-pure water is not colourless, as is usually supposed, but naturally possesses a pure bluish tint, which is only rendered visible to the eye when the light penetrates through a stratum of water of considerable depth. That such is the fact, may easily be shewn by taking a glass tube, two inches wide and two yards long, which has been blackened internally with lamp-black and wax to within half an inch of the end, the latter being closed by a cork. Throw a few pieces of white porcelain into this tube, which, after being filled with chemically-pure water, must be set vertically on a white plate, and looking through the column of water (of two yards) at the pieces of porcelain, which can only be illumined from below by white light, we shall observe that the objects will, under these circumstances, acquire a pure blue tint, the intensity of which will diminish in proportion as the column of water is shortened, so that the shade of colour becomes at length too faint to be perceived. This blue coloration may also be recognised when a white object is illuminated through the column of water by sunlight, and seen at the bottom of the tube through a small lateral opening in the black coating. The blue tint so frequently observed in water cannot, therefore, be regarded as in any way strange. The question therefore arises, why this blue colour is not seen everywhere, and why it should not occur in many seas? Why, for instance, the lakes of Switzerland, the waters of the Geysers in Iceland, and in the South Sea Islands, should exhibit every shade of green, whilst the waters of the Mediterranean and Adriatic are occasionally of so deep a blue as to vie with indigo? These questions are easily answered, since clearness and depth are the primary, if not the sole requirements for imparting to water its natural colour. Where these fail, the blue tint will likewise be wanting. The smallest quantity of coloured elements which the water may take from the sand or mud of its bottom, the smallest quantity of humus held in solution, the reflection of a dark and strongly-coloured bottom, are all sufficient to disguise or alter the colour of water. It is well known, that the yellowish-red colour of the waters which traverse the lower group of the trias formations depends upon hydrated oxide of iron, contained in the mud of the variegated sandstone. From a similar cause, the vast glacier streams of Iceland, which, in these desolate regions where there are neither roads nor bridges, the traveller finds, to his discomfort, that he must ford, are rendered opaque and milk-white from the detritus of dark volcanic rocks, which, crushed into a white powder by the overwhelming mass of the descending glaciers, are carried to the sea, in the form of white mud and sand, and again deposited there in vast deltas.

In like manner, the natural colour of the small lakes in the marshy districts of northern Germany is concealed by the black tint imparted by the dissolved humus derived from the turf. These waters often appear brownish or black, like the water in most of the craters of the Eifel and Auvergne, where the sombre volcanic rocks obstruct the reflection of the incident light. It will, therefore, easily be understood that it is only where these disturbing influences do not exist that the colour of the water will be seen in all its beauty. Amongst the places at which this requirement is most completely fulfilled, we may especially instance the Blue Grotto at Capri, in the Gulf of Naples. The sea is there most remarkably clear to a very great depth, so that the smallest objects may be distinctly seen on the light bottom at a depth of several hundred feet. All the light that enters the grotto, the entrance of which is only a few feet above the level of the sea, in the precipitous rock opening upon the surface of the water, must penetrate the whole depth of the sea, probably several hundred feet, before it can be reflected into the grotto from the clear bottom. The light acquires, by these means, so deep a blue colouration from the vast strata of water through which it has passed, that the dark walls of the cavern are illumined by a pure blue radiance, and the most differently coloured objects below the surface of the water are made to appear bright blue.

An equally remarkable example of this fact presents itself in the glaciers of Iceland, as well as in those of Switzerland, which shews that water does not lose its original colour even when in a solid condition. At the distance of many miles, the eye may distinguish, on the flat heights of the "Jokull," the boundaries that separate the bluish ice of the glaciers from the white inaccessible plains of snow that rise to the summit of these mountains. On a closer examination of these glaciers, one is surprised to observe the purity and transparency of the ice, which often appears to be wholly free in large masses from vesicles of air and foreign admixtures, whilst its vast fissures and cavities are coloured all shades, from the lightest to the darkest blue, according to the thickness of the strata through which the light has penetrated.

The blue tint of the cloudless and vapoury atmosphere is probably dependent on similar phenomena, if we are justified in concluding, from the colour of solid and fluid water, that aqueous vapour has a similar colour. On considering all these facts, we can scarcely doubt for a moment that the blue colour of water is a peculiar and not accidental characteristic of that substance. This natural colour of water will also afford us an easy explanation of a light green tint which is even more strongly manifested in the crystal-like siliceous springs of Iceland than in the lakes of Switzerland; for the yellow colour derived from traces of the hydrated oxide of iron, in the siliceous sinter walls surrounding the water, blends with the original blue to produce the same greenish tint, which, in the Swiss lakes, is derived from the yellow bottom; the most different rocks experiencing a superficial decomposition from the continued action of water, and becoming tinged with yellow by the formation of hydrated oxide of iron. Hence it will be easily conceived that the blue, which continues to increase in intensity with the increased depth of the strata of water, may obliterate the action of this yellow reflection, and thus either weaken or wholly destroy this greenish tint. The green grotto on the shores of Capri affords a most striking proof of this fact. The green colour, which is produced by the reflection at an inconsiderable depth of water, from the yellowish limestone constituting the bottom and the walls of the grotto, illuminated by the light from without, wholly disappears in the enormous depths of the water of the blue grotto; there a pure blue colour takes the place of the green, observed in the shallower cavern, although the water and rocks are the same in both cases.*

Geological Changes from Alteration of the Earth's Axis of Rotation.†

Respecting a possible change of climate resulting from a change in the earth's axis of rotation,—an hypothesis which has from time to time engaged attention, as one which might serve to account for the occurrence of organic remains, supposed to be those of animals and plants requiring a higher temperature than that of the regions where such remains are found, we have had two communications. In one from Mr Saull, he calls attention to the undoubted evidences of the land being at intervals above and beneath the waters, and to changes of temperature over the same area. These effects he attributes to a change in the earth's axis of rotation, arising from astronomical causes, and describes the results which would follow from such conditions. As to the possibility of a change in the earth's axis of rotation, we had a paper from Sir John Lubbock, in which he first adverts to the revolution of a solid body on its principal axis, and its continuing to do so for ever, unless such solid body be acted upon by some extraneous force. He further observes that on this supposition no change of climate would obtain on any given latitude on the earth's surface, except from a change in internal temperature or the heat of the sun.

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* *Vide* Works of the Cavendish Society, vol. i., 1848.

† Sir H. J. De la Beche's (President of the Geological Society) Anniversary Address to the Society for 1849.

He then notices that a change of climate alone is not sufficient to account for geological changes, such as water covering a part of the earth's surface at one time and not at another: and remarks that the moon's attraction and the causes which produce the precession of the equinoxes do not modify these conclusions.

Sir John Lubbock then states, that "it is unlikely that when the earth was first set spinning, the axis of rotation should exactly coincide with the axis of figure, unless indeed it were all perfectly fluid." He subsequently takes a period not so remote, when the earth, from the different fusibility of its component parts, might have been partly solid in irregular masses and partly fluid, and afterwards a still more advanced state, in which land and water irregularly occurred on its surface, suited to the existence of animal life, always supposing the axis of rotation not to coincide with the axis of figure. If any resistance exists, "the pole of the axis of rotation would describe a spiral round the axis of figure, until finally it would become, as at present, identical with it." Supposing a displacement of the axis, the movement of the water from one equator to another and the consequent changes of climate are pointed out. Glancing at friction on the surface of the earth rendering the invariability of geographical latitude, otherwise existing, not a necessary consequence,—at our ignorance of the earth's structure beneath its crust,—and of the history of the changes effected during the process of cooling,—Sir John considers that "the utmost that can be accomplished by mathematics is to explain under what hypothesis a change of the position of the axis of rotation is possible or not." Adverting to the dictum of Laplace, that the changes on the earth's surface and in the relative positions of land and water cannot be accounted for by a change in the position of the axis of rotation, he observes that in this statement Laplace did not take into consideration either (1.), the dislocation of the strata by cooling, or (2.), the friction of the surface. Finally, our colleague, after admitting that if at any remote period the earth had been a homogeneous spheroid of any pure metal in a state of fusion, it would in cooling always revolve about the principal axis of rotation, that of figure, considers that there is sufficient evidence of want of homogeneity on the earth's surface to bring a change of axis of rotation within the limits of possibility.

It is always gratifying to find mathematicians so far interested in our science as to occupy themselves with the solution of problems, which, when we consider their important bearing, scarcely seem to occupy the attention they would appear to deserve. The early condition of our planet is one of these. By carefully considering the possible and probable conditions connected with that state, we dismiss or retain, as the case may be, much that is of great importance in theoretical geology. Hence the value of such communications as this before us, wherein the conditions for a possible change in the earth's axis are considered. As you are familiar with the reasoning founded on the figure of the earth, it is merely necessary to remind you of its bearing upon the original fluidity of our planet, a fluidity which there has been a difficulty in referring to any other cause than to a heat sufficient to keep the component particles asunder, in such a manner that even to the centre of the mass the pressure was insufficient to prevent a free motion of the particles of matter.

Sir John Lubbock would appear to have adopted the idea of a cooling body, but referring to the want of homogeneity observed among the parts of the earth thrust up into the atmosphere, and known to us, he calls attention to the effects which might follow this want of homogeneity in our globe. It hence becomes important to learn the value which can be attributnd to such a cause. The depth to which we may limit that portion of our spheroid, which is formed of such substances as we find composing masses of rock exposed to our examination, is necessarily very difficult to fix. The highest mountains, rising even in the warmest regions of our globe so far into the atmosphere as to feel the influences of the low temperature surrounding our planet, however vast they may appear to us, merely give a few miles of thickness; and when we fairly estimate the real depth of the various ascertained accumulations of different geological ages, we still arrive at such an insignificant portion of the earth's radius, as to see how very little of the component parts of its mass can be known to us. Still we are bound to examine the evidence as to the differences which may exist as regards homogeneity in the rock masses. Some years since (fifteen), having occasion to estimate the probable specific gravity of fifty miles in depth of the earth's crust,* we found, from direct experiment upon such rocks as appeared important, that these varied from 2·49 (chalk) to 3·03 (diallage rock from the Lizard, Cornwall). Upon estimating the masses, taking the surface into consideration, and, therefore, probably giving more differences to the depth supposed, fifty miles, than should be allowed, the mean specific gravity came out as 2·59 higher than the density of 2·5, that commonly adopted, and yet sufficiently near that density for the purpose intended.

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* Researches in Theoretical Geology.

Laplace estimated the mean density of our planet as 1·55, the solid surface being considered as 1, hence taking the interior density higher than that of the external parts. We see, looking at such mineral substances as form masses of rock, that they are all oxides; but of the depth to which these oxides may descend we know nothing. Unless we suppose them oxides from the beginning, that is, from the time the matter of our earth may have been gathered together as a body revolving around the sun, an hypothesis for which it would appear difficult to find much reason, the various metals, such as silicium, aluminum, calcium, and the rest, became oxides from coming in contact with oxygen. We have sufficient oxygen in our atmosphere, supporting the animal and vegetable life which now exists, and which probably also during a long lapse of geological time has existed on the earth's surface, to permit the assumption that in an early state of our globe oxygen may readily have been far more abundant among the gaseous portion of the matter forming our planet, including its atmosphere, than at present, when animal and vegetable life is adjusted to the quantity remaining.

As far as we are acquainted with the substances forming our globe, we may have an oxidized solid crust, supporting in parts a comparatively thin and irregularly-disposed covering of saline water, and enveloped by a gaseous covering, the interior not composed of oxides, but more or less homogeneous, allowing for the effects of any heat, which may be supposed to remain in it, and for the densities due to the gravitation towards its centre of all the particles of matter of which the earth is composed.

When we have to consider any changes in the earth's axis of rotation due to the absence of homogeneity in its component parts, we have also to regard the probability of this want of homogeneity extending to a depth at which it would have any appreciable value. As far as the distribution on the face of the earth of the igneous rocks is known to us,—rocks whence, with the exception chiefly of limestone deposits (many of which have been accumulated by means of animal life), so many others have been formed,—we do not find any accumulation of masses of very different density in one part more than another, so as to have produced very marked differences in density on at least the surface of our spheroid. On the contrary, we find the probable distribution of granite and granitic rocks with the same density, very uniform in various parts of the earth's surface, and their abrasion has furnished abundant materials for other rocks. The like happens with the heavier compounds of hornblendic and felspathic substances, and the strata derived from them. Masses of limestone are indeed here and there more irregularly distributed; but as the limestones do not differ much from the granites in specific gravity, no great effects would follow their unequal distribution, more particularly when we take into consideration the small depth to which they would probably descend in the earth's crust.

We have also to regard the effects arising from the dislocation of the strata, as noticed by Sir John Lubbock. There are few geologists who are not now prepared to admit that the surface of the earth, since we may assume any solidity in that surface, has been in an unquiet state, some large areas moving upwards, some downwards, and these movements sometimes repeated in the same area; deposits crushed and folded against each other here and there in long lines, so that parts of them are thrust high up above the level of the sea, while masses of accumulations are forced asunder in other situations, and mineral matter raised from beneath occupies parts of the area over which they previously spread. Up to the present time mineral matter is here and there vomited forth in fusion, or blown out of vents by the discharge of vapours and gases, and large tracts of the solid surface of the earth are violently shaken, and portions of land raised or depressed. We also know that at the present, slow changes in the relative levels of sea and land are being effected. Thus from our own experience and from the study of what has formerly happened, we find that the surface of our planet is and has been, during the lapse of such geological time as we can trace, in an unquiet state. We of course know nothing of the height to which the crushing or elevating of rocks into mountain-chains may have forced mineral accumulations, though we may often infer that very great heights are but the remains of rocks, the removed portion of which rose still further into the atmosphere; but, taking the Himalayan chain as the highest land, we have nothing rising six miles above the sea-level. If we increase this height to ten miles, we should still have an insignificant fraction of the earth's radius.

The researches of Mr Hopkins lead him to infer that at present the solid crust of the earth cannot be less than 800 to 1000 miles thick. Supposing this to be so, the hypothesis of a cooling globe would give a less thickness in past geological times, one gradually diminishing to the early period when solid matter could be first formed. I need scarcely call your attention to the view which has been taking of the forcing-up of mountain-chains, and the unequal tilting and adjustment of masses of the surface to accommodate the crust to the still fluid mass beneath, as cooling proceeded. Neither need I speak of the effects which would follow from the action of the heated and still fluid mass upon the portions of the fragments which may have descended different depths into its surface, or of the intrusion of the molten matter amid the broken masses; we have only to inquire how far these breakings-up and squeezings of the previously solid crust at different times is likely to have interfered materially with its general uniformity, so that any important change in the earth's axis, with its geological consequences, may have resulted.

As regards the mineral matter thrust up into the atmosphere, we see that, as soon as this is effected, it is attacked along the sea-level by the breakers, and both on coasts and inland by atmospheric influences, all tending to lower the altitude of the mass so elevated, and to carry its component parts into the sea, filling up any inequalities which may have been formed beneath it, in consequence of this surface-movement of the rocks. It is during this removal of mineral matter and its spread in various directions, that the remains of the animal and vegetable life of succeeding geological times become entombed, adding, and in many instances most materially, to the masses accumulated in various ways upon the previously moved rocks. This action, therefore, tends to plane down the unequal surface above the sea and fill up inequalities in its bed. While this proceeded, we should expect that the heated matter beneath would also melt down any portions of the solid masses, squeezed and forced into it by these movements, to a distance from the surface corresponding with the general heat of the globe at the time, and therefore the deeper as geological time advanced and the earth gradually parted with its heat, by radiation, into surrounding space.

Under this view there would be a tendency over the face of the globe to retain a general crust upon it of a thickness increasing with the lapse of geological time, less uneven beneath, as a whole, than above, from the kind of action to which it would be subjected, and yet no part protruding so far as to cause any very material difference in the figure of the earth, or of density, in the parts of such crust. Viewing the subject on the large scale, it would not appear improbable, that notwithstanding the dislocation, unequal tilting, and squeezing together of masses, the adjustments were such as to keep a spheroidal coating of the mass beneath, which did not very materially differ as a whole in density. Should this not have been so, we have in our geological hypotheses to take into account the effects pointed out by Sir John Lubbock as resulting from the modification or absence of the general conditions above inferred, their amount or geological value necessarily depending upon the magnitude of the causes to which he adverts.

14

On the Downward Progress of the Glaciers of the Alps.

By Ed. Collomb.

Glaciers being the definite result of meteorological and climatological phenomena, their secular encroachment upon the lower valleys of the Alps may serve as a term of comparison to determine the changes that have taken place in the climate of the country.

This encroachment may take place in two ways, either by the progression of their frontal portion, or by the swelling out of their lateral parts.

We may have the case of a glacier with the frontal part alone advanced forward, without the parts situate towards the middle undergoing any dilatation. The reverse of this may, on the other hand, present itself; that is to say, the terminal talus may remain many years nearly stationary, and yet we may observe a sensible expansion of the surface in the middle region. Glaciers, therefore, exhibit two modes of proceeding in encroaching upon the land, one frontal, the other lateral.

These phenomena depend on three causes which act incessantly on the physiology of glaciers, if we may use such an expression. These are the *alimentation, movement, and ablation.*

The alimentation of glaciers, in other words the cause of their existence, is to be found in the quantity of snow which falls in the whole zone of the Alps situate above from 2800 to 3000 metres. At this altitude, the solar or ambient heat is insufficient to melt the snow that falls in the course of the year. In these high regions the alimentation exceeds the melting power, and if a movement did not take place from the first origin of the glaciers, at the end of a few ages such an accumulation of névé would ensue in these regions, that the conditions of existence in which the valleys of the Alps now are, would be completely changed.

At a height of 3000 metres, about 17 metres of snow fall annually; and its primitive density, according to M. Dollfus's experiments, is 85 kilogrammes* the cubic metre. By sinking, pressure, and evaporation, these 17 metres become reduced to 5 metres of névé, or ice of névé, the density of which is 250 kilogrammes the cubic metre. During the three warmest months of the year, or rather the three in which there is least cold, the ablation does not exceed one metre of the surface; there remain, therefore, four metres every year, which the sun cannot melt at that altitude. If no movement took place in these four metres, if the property they possess of moving onwards, and gradually reaching the lower regions, were taken away, and they were thus rendered immoveable, four new metres would be added every year to those of preceding years; and even during historical times, the névés accumulated in the upper regions would surpass the height of the most elevated summits of the chain.

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* Kilogramme is equal to 2 pounds 4 ounces avoirdupois.

Thus, by reference to facts, we obtain a proof that the movement alluded to takes place on all the cols or passes and most elevated summits of the Alps, wherever the heat of summer is not sufficient to melt the entire snows of winter.

This movement, therefore, has its point of departure in the circuses, passes, and summits; it commences at the upper limit of all the basins of the glaciers, propagates and develops itself throughout their whole mass, and the whole extent of their course; very feeble at first, it gradually acquires some degree of speed in the middle regions, and again becomes slower in the lower regions. All the snows above 3000 metres being destined by nature to be melted, that they may not accumulate indefinitely, they are made to descend to the lower regions where they are exposed to a warmer sun which allows them to dissolve into water.

The alimentation and movement are, therefore, two forces, one of which accumulates the snows of the higher regions in a vertical direction, and the other spreads them over a greater surface in a longitudinal direction. If these two forces existed alone in nature, nothing would arrest the glaciers in their downward progress, and they would invade all the valleys of the Alps; but a third important element is added to the two others, namely, *ablation;* it is proportionate to the surrounding temperature, and acts in the inverse ratio of altitude. In the upper regions there is scarcely any ablation;

it does not, as we have mentioned, carry off as an annual mean, much above one metre of the surface of the névés; in the middle region it carries off two to two-and-a-half metres of the ice; and in the lower regions of the glaciers, three to three-and-a-half metres; consequently, all the bodies of ice which the movement carries from above downwards, are successively exposed to a stronger ablation.

To recapitulate what has been said regarding the three causes which concur in the mechanism of the formation of glaciers, we may state:

1*st*, The alimentation and movement of themselves would cause an extraordinary extension in the length.

2*d*, Alimentation and ablation alone would produce an indefinite extension in a vertical direction; the glaciers would not descend into the valleys, but continue in the higher regions.

3*d*, Movement and ablation, apart from alimentation, would put an end to their existence.

It is the combination and influence of these three causes which regulate the glaciers, and maintain them in their present state; this is the law of their existence, the condition of their being.

These preliminaries were necessary to enable us to perceive the force of the observations which follow; the more so, since we know that this equilibrium between the natural forces which confine glaciers within their present limits, has not always existed on the surface of the earth. At a period of its history comparatively recent, geologically speaking, the equilibrium has been disturbed, the alimentary force has prevailed over the melting force, and glaciers have acquired a considerable development.

In present times, since attention began to be paid to glaciers, we find that they have been subject to perpetual changes: some years they are advancing, in others retreating; or one glacier may be advancing while a neighbouring one is receding. As a general rule, the front of glaciers advances in winter, and recedes more or less during the warm season; because, in the winter there is no ablation, alimentation and movement alone continuing to exert themselves. Yet we are about to make an endeavour to shew that glaciers, taken as a whole, are not stationary: going backwards many centuries, we find, as a definitive result, that they encroach slowly, and in the course of an age, upon the lower valleys. We shall enter into some details, and cite some examples in support of this opinion, derived from notes made on the spots last summer.

The glacier of Aletsch, the largest of all, about 24 kilometers* long, and 110 square kilometers of surface, forms a mass approaching to 30 milliards of metrical cubes of ice; its direction is from north to south, its lateral expansion in the course of ages produces very remarkable effects.

On the left side, it is bounded by a chain of mountains which is a continuation of the Æggishorn. These mountains are partly covered by a very dense forest of pines, and, for a space of 4 kilometres of its length, measured from the terminal talus upwards, the glacier ravages and destroys a great number of these trees. The left lateral moraine reaches them, and attacking them first by the roots, the tree falls, and is carried along by the motion of the ice. Those which are caught between the ice and the boundary rock are speedily torn in pieces, while such as fall on the surface share in the general movement, but are not long before being dragged under the glacier. At the terminal talus we observe them issuing from below the masses of ice, some half entangled, others completely free, the latter pushed forward and precipitated into the torrent. All these trees are completely stripped of their bark and torn, nothing remaining but the principal trunk and some of the strongest branches crushed and twisted.

With regard to the age of these pines, it may perhaps be estimated at a minimum of 200 years; they are large, strong, and thick, and it is well known that, in these elevated regions, at the limit of arborescent vegetation, pines continue many years before attaining a large diameter. There must, therefore, have been a period of at least 200 years during which the glacier did not reach the margin of the forest which it now lays waste.

If we pass from the left bank to the right, we also find proofs of its dilatation. On a lateral piece of land, situate a little below the tributary of the Unter-Aletsch, rich pasturages still exist, which the inhabitants of the country formerly turned to profitable account. The road leading to this locality ran along the foot of the mountain, leaving the glacier at a distance; now, the right lateral moraine has destroyed the road, and advanced to the foot of the mountain, so that the passage has become impracticable. But, as they were unwilling to lose these fine pastures altogether, horses and mules are still sent to them, along a road cut with hatchets in the glacier itself, but this mode of communication is not practicable for cows.

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* *Kilometer* is equal to 1093⅔ yards.

Some kilometres further down on the same side, there is another lateral spot equally rich in pasture, and on which we observe twenty-four wooden houses scattered about; these houses were formerly inhabited, and formed a village which bore the name of Aletsch. For several years back many of these houses have been destroyed by the lateral intrusion of the glacier; they no longer serve as permanent habitations, but are converted into barns, some of them only being inhabited for a few months of the year. At the time when we examined the spot, one of these houses was just about to be overwhelmed by the stones and enormous blocks, detached from time to time from the moraine, which had nearly enveloped the frail edifice.

The erection of a village at this point goes back to a very remote period. Although the inhabitants of the country could give us no information on this point, still, it is obvious, that the first inhabitants who took up their residence here, would never have been guilty of the folly of building permanent houses, with the prospect of seeing them every instant swallowed up by the glacier. We may, therefore, conclude that the glacier, at that time, was at a great distance from the village, and by reckoning it to have been built 200 years ago, we remain within the minimum. The same thing, then, has taken place on the right as on the left bank; the glacier, by its increase, now arrives at localities which it had not touched upon for many ages.

In the glacier of Zmutt, in the valley of Zermatt, it is rather a frontal progression than a lateral expansion that we have observed. This glacier is partly fed by the snows of the northern acclivity of Mont Cervin. Its surface is covered with rocky debris ; its lateral and median moraines are very large, and they mingle and expand in a fan-shape, so as completely to cover the glacier; the ice disappears under this mass of rubbish ; but these circumstances are favourable to a rapid advance, and it penetrates far forward into the valley. The advance was so great in 1848, that it laid waste a forest of larches, overthrew and destroyed large trees, whose age was estimated at 300 years, independently of a great number of dead larches which it pushed forward along with the stones, blocks, and sands of the frontal moraine ; it surrounds, on all sides, an islet of rocks from 35 to 40 metres in height, on which three large larches, from 25 to 30 metres, are still standing and vegetating. They stand on the rock like three condemned sentinels ; the attacks of the cold are sensibly felt, and although they are living, half of their branches are already dead.

It is evident that these larches have not taken root on the rock since it became surrounded with ice ; a medium so cold is not favourable to vegetation. On the other hand, if the glacier carried before it trees 300 years of age, it follows that, in this valley, the ice has been advancing for three centuries.

In this same valley of Zermatt, the glacier of Gorner, which descends from Mont Rosa and the Lyskamm, advances in a way disastrous to the proprietors of lands situate towards the front of the glacier. The meadows immediately in contact with the moraine are ploughed up, the turf raised in rolled masses a metre in diameter. Many of the habitations in this locality are abandoned, and serve only as barns for preserving fodder. It is a fact well known to the people of the country, that, about fifty years ago, twenty barns, stables, and inhabited houses, existed in a locality on the left bank, which is now completely covered with ice. The house nearest the moraine on this side, was distant 8 metres in August last ; those who built the house did not suspect that this formidable neighbour would approach so near. I likewise observed, among the materials of the frontal moraine of

Gorner, in the midst of blocks of serpentine, gravels, earth, and sand, fragments of large trunks of pines, which the glacier had torn from the forest at the foot of the Riffelhorn, a kilometre distant.

It is, then, with the glacier of Gorner as with the preceding; it has been in a state of progression many ages.

The glacier of Viesch, on advancing into the valley of the same name, encounters a formidable obstacle of rocks *in situ*, which form a promontory in the middle of the ice, and forces it to diverge to the right and left. This promontory is covered by a forest of pines a century old, but the ice-fields always advance and destroy a great quantity of trees every year; between the forest and glacier, there was this year a zone of pines intermingled with the debris of rocks forming part of the lateral moraine.

The guides of the place assured me, without mentioning exactly the period, that a village, called Auf der Burg, formerly existed not far from the obstacle just spoken of. Of this village not a trace remains; the ice has overwhelmed all.

According to M. Vénetz, the inhabitants of this valley were accustomed to communicate, three centuries ago, with those of the Grindelwald. There was a chapel on the upper pass, the bell of which has not been lost, being preserved at Grindelwald. This passage is now one of the most perilous among the Alps, and great experience and skill in glaciers are necessary to enable one to accomplish it.

In regard to the glacier of the Aar, M. Agassiz concludes, that an advancement of 800 metres has taken place in a hundred years, according to documents obtained from a work of Altmann in 1751.* Since M. Agassiz' departure, M. Dollfus and myself have attentively watched the progress of this glacier, and have observed this year a fact very conclusive as to its advancement. On the left bank, in a locality indicated on M. Agassiz' map by the name Brandlamm, there exist on the sides of the enclosing mountain a few rugged last summer stumps of *Pinus cembra;* one of these pines has been reached by the glacier; we have sawn the trunk and ascertained the age, which is 200 years. By the side of this trunk we observed ancient beds of the same substance, passed into the state of rotten wood, which must go back to a more remote period; but it was impossible to determine its precise age. All the observers who annually visit this glacier, observe that the pines which still grow on the sides of this mountain are gradually disappearing, and in a few years none will remain.

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* Agassiz, *Nouvelles Etudes sur les Glaciers*, p. 542.

At two kilometres further down, on the road from the glacier to the hospice of Grimsel, a small peat-moss has been dug, and the workmen frequently find there, at the depth of a metre, old trunks of pines of very large diameter, such trees as could not grow there in the present temperature of the locality.

At the pass of Saint Theodule (3111 m.), there is an old military structure which goes back as far as the time when Lombardy was occupied by the Spaniards. It is a dry wall, coarsely built with slabs of gneiss and slaty serpentine, and pierced with loopholes turned towards Switzerland. In order to reach this pass, whether on the south or north side, we walk for an hour and a half over glaciers of very difficult access, especially on the side of Italy. At the time when I passed it, in the month of September, the fresh snow was all gone, all the névés were conveniently hardened. The establishment of a military post at this point would be inexplicable, in consequence of the difficulty of the passage even in the most favourable season. On visiting the spot, we cannot but suppose that at the time when the redoubt was built, the passage was not only more frequented, but of much easier access, and that for some ages the pass has become more encumbered with ice.

The greater part of the glaciers on the southern acclivity of Mont Blanc are likewise in progress; that of La Brenva has advanced thirty-one metres this year, according to the statement of M. the Canon Gal.

We might multiply examples; but the preceding facts are sufficient to demonstrate the advancement in the course of ages of the glaciers of the Alps: if there be any of them which recede, this is only an exception to the general rule.

The glaciers we have mentioned, are situate at points widely remote from each other; some form part of the group of the Jungfrau, others of the group of Mont Rosa, the last of the group of Mont Blanc. Some run from south to north, others from north to south, others from east to west. All are included within the parallels of 45° 45′, and 46° 35′ north. Some are protected by superficial moraines, in others these are insignificant.

Must we conclude from these facts, that we are advancing towards a slow and continuous sinking of the mean temperature of our hemisphere? This conclusion would be premature; it would be opposed to the recent and skilful observations of MM. Dureau and Malle *on the Comparative Climatology of Ancient and Modern Italy*,—observations intended to prove, that " since the age of Augustus up to the present period, the climate of Italy has undergone no sensible modifications in the annual, and even monthly, mean temperature."* MM. Dureau and Malle having assured themselves, that " in regard to the same places and the same altitudes, the periods of sowing, flowering, mowing, harvest, ripening, and vintage, were almost the same in ancient as they are in modern Italy, we think we have it in our power to deduce the duration of the cycle in which the complete work of annual vegetation took place, and to obtain from it the proof of the constancy of the climate of Italy during twenty centuries." †

MM. Dureau and Malle have taken their examples from vegetation. It is indeed immediately dependent on meteorological causes; it may give valuable indications respecting climatology, thermometrical means, and the secular changes which take place in the ambient medium.

But glaciers are not to be overlooked; they ought to enter as a considerable element into the solution of the question; their advancing or retrograde movements depend on the same atmospheric causes. It has been said of glaciers, that they may be compared to gigantic natural thermometers; in cold and moist years they descend into the valleys, in warm seasons they ascend towards the snowy peaks.

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* Comptes Rendus de l'Acad. des Sciences, t. xxvii., p. 356. † Ibid., p. 333.

We have seen at the outset, that their reason of existence is subject to three conditions, which consist of alimentation, ablation, and movement. The two first are essentially meteorological; the third, the dynamic force, is nothing more than a property of matter. Passing over it, it only remains for us to inquire what may have been the modifications that have taken place in the alimentation and ablation.

The fluctuations in the alimentation, or in the quantity of snow fallen on a spot in a given time, are not directly connected with a lowering of the temperature; they do not necessarily imply a variation in the thermometrical mean. More snow may fall in a medium whose temperature maintains itself between +1° and −12° C., than in that where the temperature remains within the limits of −12°−24° C. We know that the coldest winters are not the most snowy.

It would not be a sinking of the temperature, therefore, that we would have to regard as the origin of the superabundant alimentation of glaciers; it would arise from a more considerable evaporation in the low and warm regions, because it is the hygrometrical state of the air, or the quantity of vapour which is converted into snow on the great condensers of the Alps, which is the primary cause of the alimentation.

The melting or ablation is in direct ratio with the temperature, as the experiments of M. Agassiz on the glacier of the Aar sufficiently demonstrate. If we could succeed in shewing that the secular advancement of the glaciers arose from a less active ablation in the present times, we might thence immediately conclude, that the mean summer temperature has sunk for some ages, but this conclusion would apply only to the fraction comprehending the four warmest months of the year; during the eight other months, the ablation scarcely amounts to anything, because during this period the glaciers are covered with a mantle of fresh snow, which protects them from melting.

From the preceding facts, we may perceive that the problem is very complex; yet, if we set aside useless terms, it is only in the study of meteorological phenomena that we can find the solution of it; it may even be found in two propositions which may be combined or regarded singly, and may be briefly expressed in the following terms:—

1*st*, That the heat of the summers is no longer sufficient among the Alps to arrest the progression of the glaciers into the lower valleys.

2*d*, That the winters, without exactly being colder, produce in our day a greater quantity of snow than in past ages.*

On Trees cleft by the direct action of Electrical Storms.

By Ch. Martins.

The passage of electrical storms over the wooded parts of the country is marked by varied effects on the trees which cover it. A great number of them are merely torn up and thrown on the earth, others are uprooted and transported parallel to themselves to great distances. A great number have the heads broken off, and the country is strewed with branches and twigs broken and scattered to a distance. All these effects are well explained by the action of the violent wind which drives the clouds charged with the electricity constituting the electric storm. It is not the same with the *cleft* trees of which we are about to speak. The action of the wind cannot explain the appearances which they present. At leaving the ground, or more frequently from 2 to $0^{m}{\cdot}50$ from the ground, and for a length varying from 2 to 5 metres, these trees are divided into laths, in shreds or splinters, often as small as matches. The Society may observe this in the numerous trunks which I now exhibit, and which were cut in the neighbourhood of Montville and Malaunay after the celebrated storm of the 19th August 1845. This cleavage never extends to the whole of the tree, but only the half or three-quarters of its thickness. The cleft part is turned sometimes to the side from which the meteor came, at other times to the opposite side. The tree is broken in the middle of the length of the cleavage, and the top is not carried off as in the decapitated trees.

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* Biblioth. Univ. de Genève, Jan. 1849, p. 30.

A still more essential character is, that these laths and splinters are completely dried immediately after the passage of the meteor. M. Preisser assured himself of this at Montville; MM. Decaisne and Bouchard in the trunks struck by the storm at Chatenay; M. de Gasparin in the poplars broken by the storm of Courthezon. The dryness of these splinters renders them extremely fragile. M. d'Arcet found only 7 parts in 100 of water in the cleft trunks of Chatenay. Now, standing trees contain from 30 to 40 parts in the 100; and such as have been felled for five years still contain from 24 to 25 parts in 100. The bark of the cleft trees is split, torn, rolled upon itself, and cut into shreds, adhering to the tree or scattered around it.

A fact related by M. Boussingault perfectly explains this evaporation of the sap under the influence of electricity. On the 22d May 1842, the lightning fell upon a large pear-tree, at Bechelbronn in Alsace; a thick column of vapour, like the smoke which issues from a forge fed by coals, arose, and splinters of wood were thrown to a distance of many metres; the bark had disappeared; the tree appeared entirely white. M. Boussingault does not doubt that it was the vapour of water which made this tree fly in pieces. I am entirely of this opinion. It appears to me that cleft trees may be compared to boilers burst by the expansion of steam.

In cleft trees the sap mostly evaporates, the trunk is split into a thousand pieces, and the wind acts on the cleft portion, which evidently offers less resistance than the rest of the trunk. This evaporated sap resembles thick smoke, hence the mistake of the onlookers at Montville, who all supposed that a fire had broken out in the forests over which the storm passed.

The deep colour of the evaporated sap was probably owing to the earthy particles which the wind and the electrical attraction raised into the air. Lastly, to finish the demonstration, MM. Becquerel, father and son, have succeeded in reproducing, by means of strong electrical discharges, cleavage of trees, in branches of the size of the little finger.

The cleft trees produced by the direct action of the electrical cloud mark out to us its line of passage above the ground; they always occupy the centre of the ravaged zone. On the plateau of Malaunay, its total breadth was 220 metres; in the centre they occupied a width of 89 metres.

The cleavage presents different characters in different trees. It is in oaks that it is most perfect; the tree is divided in laths which, towards the interior, are often not larger than small flexible baguettes, or even common matches. The direction of the cleavage always corresponds to that of the medullary rays; the tree being always broken across towards the middle of the length of the cleavage, the baguettes which can be detached are in general only the half of their entire length. I have separated two from the upper broken part of an oak which are, the one $2^{m}\cdot50$, the other $2^{m}\cdot27$ in length. The first measured eight, the second five millimetres on the side.

In beech-trees, the cleavage is coarser than in oaks; we rarely observe matches; they are laths always two or three centimetres broad, but often very long. It was in a large beech $0^{m}\cdot38$ diameter at the base, that I observed the longest cleavage; it began at the surface of the ground, and rose upwards to $7^{m}\cdot50$; the tree was broken in the middle of this length. Beeches were likewise the only trees, some of which, four in number, remained standing after having been cleft from the surface of the ground for a third or fourth of their circumference, up to a height of from two to five metres. These trees in every respect resembled trees struck with lightning.

The cleavage of poplars differs much from that of the trees we have mentioned; instead of being parallel, the planes of cleavage are perpendicular to the rays of the tree. The greatest breadth of the laths is in the direction of the layers of white wood, which are separated from each other and disjointed. Sometimes even the wood may be drawn out from the white wood, as we withdraw the piston from a pump.

In the valley of Montville, no resinous tree (pines, firs, larches) was cleft. I counted twenty of them more or less injured, but none were cleft, although they were in the direct passage of the storm, and surrounded with others whose unk was like a bundle of laths. Now we know that the iferæ contain little sap, but much resin, especially between e bark and wood. The resin being a body which is a very d conductor of electricity, we suppose that the fluid did t traverse these trees. This observation is a new proof t the cleavage is owing to the evaporation of the sap ted by an electrical current of great energy.

The Carboniferous Fauna of America compared with that of Europe. By Ed. de Verneuil.

When we compare the carboniferous Fauna of America with that of Europe, we see with astonishment, that, notwithstanding the distance which separates these countries, the genera and the species present the same modifications, the same differences, from the preceding fauna.*

In fact, while the persevering researches of Mr King, in Pennsylvania,† go to prove to us the existence of large air-breathing animals at this epoch, the discovery of a Saurian, recently made in the carboniferous beds in Germany,‡ proves that the appearance of this class of animals, more ancient than has been believed until now, was contemporaneous on the two continents.

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* The analogy between the two continents appears to be more marked at this epoch than at the anterior epochs, the number of identical species being relatively more considerable. If we seek the cause of this, we are led to attribute it to more analogous physical conditions, which proves the uniformity of the deposits of this epoch, and perhaps also to a peculiar disposition of the submarine outline, that is to say, the bottom and the islands which extended from Europe towards America. M. Elie de Beaumont explains this disposition in a very natural manner. He regards it as an effect of the W N W. upheaving, which preceded the establishment of the carboniferous system, and which he has called the system du Ballon of the Alsace. We are happy to see the beautiful theory of our illustrious friend thus confirmed by independent researches.

† See the interesting letter of Mr Lyell upon the evidence of the foot-prints of a quadruped resembling the Cheirotherium, in the carboniferous strata of Pennsylvania (*American Journal of Science and Arts*, 2d ser., vol. ii., p. 25).

‡ This discovery, of which we have been informed by Von Buch, destroys the principal objection which could be made to the extent which we have given to the Palæozoic formation, in our work on Russia, by comprising in it the Permian system; for this objection was founded on the opinion then established that the Saurians appeared for the first time in this system; and the importance of the appearance of animals of this class to determine the point of departure of the secondary formation.

The trilobites follow a similar order of decrease, and are reduced in America as in Europe, to small species of the genus Phillipsia. The Goniatites offer also for the first time the new type, where the dorsal lobe, instead of being simple, is divided by a small medial saddle.

The distribution of the Productus offers another remarkable coincidence. Unknown in America in the Silurian system, appearing under one or two small forms in the Devonian epoch, the species assume in the carboniferous rocks a development altogether in harmony with the facts observed in Europe. The Spirifers of this epoch present also in America the character of having the plications often dichotomous, which M. d'Archiac has already indicated in Europe,* and by which they are distinguished from those of the Devonian epoch, which have them always simple.†

As to the Terebratulæ, we will mention the interesting fact of the simultaneous disappearance of two species, the *T. reticularis* and *T. aspera*, which, during the Devonian and upper Silurian epochs, were spread with great profusion from the Altai and Ural to the Missouri. We will cite also, as simultaneous phenomena upon the two continents, the appearance of those Crinoids forming a passage to the Echinoderms, such as the *Palæechinus* or *Melonites*, the extinction of those great corals, such as the *Favosites Gothlandica*, *Porites interstincta*, &c., and their replacement by the *Chætetes* and *Lithostrotion*, nearly identical in Europe and America. The analogy between the two continents continues open to the *Foraminifera* and the plants. We have seen, indeed, that the

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* Memoir on the Palæoz. For. (Trans. Geol., vol. v., p. 319). See also Geology of Russia in Europe, vol. ii., p. 126. Von Buch, in his interesting Memoir which he has published upon Cherry Island (*Baren Insel*) has insisted, with reason, on the importance of this character, which might be thought insignificant.

† It is also only at the Devonian epoch that we find the Spirifers in which the back is divided by a slight furrow, as in *S. mucronatus* and *Bouchoidi*.

Fusulina cylindrica, so characteristic of the carboniferous limestone of Russia, occurs in the slates or siliceous beds of the coal sandstones of Ohio. And as to plants, the immense quantity of terrestrial species identical on the two sides of the Atlantic, proves that the coal was formed in the neighbourhood of lands already emerged, and placed in similar physical conditions.

With the carboniferous system terminates the palæozoic formation in North America. During all the time of its deposition, the surface was free from great disturbances. Slow and insensible oscillations had caused to emerge areas, more or less circular, of the submarine surface, where the Silurian and Devonian deposits were made, and had contracted the limits of carboniferous deposits ; but the horizontality of the beds was not disturbed. It is only after the carboniferous that an energetic force, folding and raising the terrestrial crust, gave birth to the chain of the Alleghanies. The manner in which the plications, largely undulated at first, contract, multiply, and fold over, in going from northwest to southeast, towards the metamorphic and granitic rocks, situated most frequently beyond the chain, properly so called, has been perfectly elucidated by the two Professors Rogers.*

It does not enter into our design further to extend this notice ;† having fulfilled, according to our ability, the end which we proposed, to establish a parallel between the palæozoic formations of North America and Europe. Permit us, in conclusion, to present a *resumé* of the course we have followed, and the principal results at which we have arrived. In order to make the interest and importance of this parallelism fully understood, and the light which it throws upon the knowledge of palæozoic deposits in general, we have shewn the advantageous geological conditions of North America, and how, owing to the horizontality of the beds over great extent of surface, to their concordant and uninterrupted superposition, it is possible to arrive at an absolute certainty as to the duration of species, that is to say, the point in the series where they first appear, and where they become extinct.

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* On the Physical Structure of the Appalachian Chain, as exemplifying the Laws which have regulated the Elevation of Great Mountain Chains generally. By W. B. and H. D. Rogers.

† We have only extracted that part of M. de Verneuil's notice which refers to the Carboniferous Fauna.—*Edit. Phil. Journ.*

In order to compare North America with Europe, it has been necessary for us to give a rapid glance at the groups and stages of which the palæozoic class is there composed. The differences which are presented to us in the geognostic conditions of the state of New York, and the Western States, such as Ohio and Indiana, have revealed to us the degree of importance which it is necessary to attach to these different groups. We have seen that their number, variable according to their vicinity or distance from lands emerged at the epoch of their formation, had little importance, as regards the establishment of systems founded upon palæontological characters. We have seen also, that in general the limestones are more constant than the shaly or arenaceous beds, that they form more extensive horizons, and furnish a surer guide to the geologist.*

Passing afterwards to a comparison of the two continents, we have shewn, supporting our views by geological analysis, how the American sub-stages should be grouped to correspond with the Silurian, Devonian, and Carboniferous systems of Europe. We have not disguised the fact, that the divisions introduced upon this principle did not correspond, in certain countries, with the divisions indicated by the mineralogical character of the rocks ; thus the limit between the two stages of the Silurian system, very well marked in the state of New York, is observed near the Mississippi, in consequence of the predominance of magnesian limestone ; it is the same with the Silurian and Devonian systems, the limit between which is found in the upper part of the great calcareous formation called cliff-limestone ; as well also as with the carboniferous system, in parts of the state of Ohio, where it is in contact with the Devonian psammites of Portage.

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* M. C. Prevost, in his Memoir upon the Synchronism of Formations (*Comptes Rendus*, April 1845), has clearly shewn the importance of the pelagic calcareous deposits, as compared with the arenaceous beds formed under the influences of coasts.

These mineralogical transitions, which one would expect in a country free from disturbances, would not, however, obscure the proofs of a parallel development of the animal kingdom in the two continents; for if, leaving aside the difficulties of fixing the limits between the systems, we compare the systems together, or, still better, one by one the groups of which they are composed, we acquire the conviction that identical species have lived at the same epoch in America and in Europe, that they have had nearly the same duration, and that they succeeded each other in the same order. We have endeavoured to prove that the first traces of organic life in countries the most remote, appear under forms nearly alike, at the base of the Silurian system, and that the same types, often the same species, are successively, and in parallel order, developed through the entire series of the palæozoic beds. If we have not succeeded in lifting the veil which still hides from us the cause of this grand phenomenon, perhaps at least our observations demonstrate the insufficiency of those causes by which certain authors seek to explain it. They prove, in effect, that the phenomenon itself is independent of the influences which the depth of seas* exercise upon the distribution of animals; for if, in certain countries, the Silurian deposits prove a deep sea, they have, on the contrary, in the state of New York, a littoral character. They prove, in fine, that in its general character it is equally independent of the upheavings which have affected the surface of the globe; for, from the eastern frontier of Russia even to Missouri, distant from or near the lines of dislocation, in the horizontal beds, as well as in those which are disturbed, the law according to which it is accomplished appears to be uniform.—(*The American Journal of Science and Arts*, vol. vii., p. 48.)

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* We do not pretend to say that the differences of depth in the seas had not already an influence upon the distribution of animals; it is to this circumstance, on the contrary, that we attribute the more or less local fauna which we often discover in the palæozoic class. But these local faunæ always afford some species which connect them with the epoch to which they belong. They are the exceptions (*hors d'œuvre*), which do not derange the general symmetry.

1. *Flora of the Silurian System.* 2. *Plants of the Anthracite Formation of Savoy.* 3. *Fossil Plants, as illustrative of Geological Climate.* 4. *Co-existence of Certain Saurian and Molluscous Forms at Equal Geological Times.* 5. *Phosphate of Lime in the Mineral Kingdom.*[*]

1. *The Flora of the Silurian System.*—In a memoir on the geology of the neighbourhood of Oporto, including the Silurian coal of Vallongo, Mr Sharpe furnished us with a detailed account of a part of Portugal, of which, in 1832, he presented a brief notice to this Society. After mention of the crystalline rocks near Oporto, his section shewing the granite of Oporto, covered on the WSW. and ENE. by gneiss, mica slate, and chlorite slate, he describes a band of rocks, chiefly formed of clay-slates, resting upon the eastern flank of the latter, and which, from the character of the organic remains obtained from it, he refers to the Lower Silurian deposits. The lowest part of this series is remarkable for containing several beds of anthracite, worked at San Pedro da Cora, eight miles ENE. from Oporto. Mr Sharpe states that the section is clear, and that these lower beds, which repose on chlorite slate, evidently dip beneath deposits containing Lower Silurian fossils. The upper part of the group is formed of a thick accumulation of micaceous sandstone, usually yellow, with some grey carbonaceous sandstone near the bottom. This rests on a black carbonaceous slate, among which are bands of indurated ferruginous clay, passing into clay ironstone. Beneath this comes a dark grey or black hard clay-slate, with softer chloritic beds of a pink or yellow colour in the lower part. Notwithstanding its contortion, this slate series is considered to have considerable thickness. The lower beds of the dark grey slates, and those lighter coloured and softer at the base of the series, are rich in organic remains (*Calymene*, *Ogygia*, *Isotelus*, *Illænus*, *Chirurus*, *Beyrichia*, *Orthis*, *Orthoceras*, *Bellerophon*, *Graptolithus*, and others), possessing a character from which Mr Sharpe refers these deposits to the Lower Silurian period.

Beneath these strata, in descending order, the carboniferous accumulations of San Pedro da Cora occur, gradually passing into the beds above them. These carboniferous beds consist in descending order of (*a*) red sandstone, (*b*) coarse conglomerates alternating with black carbonaceous shales, (*c*) coal, 6 feet thick, (*d*) coarse micaceous conglomerate, alternating with black carbonaceous shales, (*e*) coal, thin bed, (*f*) coarse carbonaceous conglomerate, (*g*) coal, four beds, from 2 to 5 feet thick, variable however in thickness in different places, the beds separated from each other by 3 or 4 feet of black shale, and resting on black shale, and (*h*) slates apparently composed of the debris of the chloritic schists on which they rest. The carbonaceous series is estimated at from 1000 to 1500 feet thick, and is seen on the north bank of the Douro, at Jeremunde, twelve miles from Oporto. North of San Pedro da Cora this series rapidly thins away, and disappears about a mile and a half from that place.

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[*] The interesting details and views in this article we owe to Sir Henry de la Beche's valuable Anniversary Address for 1849 to the Geological Society, a copy of which was forwarded to us by the Author.

Having given a detailed account of the rocks referable to the Silurian series, noticed by him in Portugal, Mr Sharpe refers to the beds described by Dr Rebello de Carvalha as forming the chain of the Serra de Marao, near Amarante; those mentioned by M. Schulz on the eastern side of Gallicia, by Link in the province of Tres os Montes, and by Le Play in Spanish Estremadura, and infers that these also may belong to the Silurian series.

The lithological characters of the carboniferous deposit of Vallongo, thus plunging beneath beds containing organic remains referred to the date of the Lower Silurian deposits, are important, as shewing the physical conditions under which the accumulations have been effected, and their general agreement with many other deposits, in which sheets of vegetable matter have been so formed, as eventually to have been turned into coal and anthracite, amid mud charged with carbonaceous matter and beds of shingles. Why we should not expect accumulations of the kind at this period, the fitting conditions for the gathering together of plants or their remains, either by growth on the spot or drift from their place of growth, so that they were mixed with little or no common mud or other sedimentary matter, does not appear. We find old mud accumulations, now forming black slates, common enough in some parts of the Silurian series, and there is no want of carbonaceous matter in the black slates of North Wales and Ireland beneath the whole mass of the beds commonly referred to that series.

The occurrence of the anthracite beds in the position and under the conditions stated by Mr Sharpe, would be highly interesting in itself, as shewing to what extent clean or nearly clean accumulations of vegetable matter may have been effected amid deposits in which the carbonaceous, and, we may fairly conclude, vegetable matter was generally more diffused amid mud and gravel; but the remains of fossil plants detected in connection with this carbonaceous series are still more interesting, always assuming that the sections seen by Mr Sharpe are unequivocal, as his certainly would appear to be, unless we suppose a most enormous reversal of these deposits.

The remains of the plants found by Mr Sharpe were submitted to the examination of our Foreign Secretary, Mr Bunbury, who, though the specimens of ferns were in bad preservation, considered that one bore a strong resemblance to *Pecopteris Cyathea*, of the coal-measures; another reminded him of *Pecopteris muricata*, and a third of *Neuropteris tenuifolia*. Mr Sharpe calls attention to the evidence, as far as it goes, afforded by these plants, of a vegetation having existed similar to that of the coal-measures at a geological date long anterior to them. It would indeed be of the greatest geological importance to arrive at an insight into the kind of vegetation that clothed the land, which furnished by its disintegration, abrasion, and removal, by river and breaker action, into fitting places of deposit, those thick accumulations now known as the Silurian series. We appear to have fair reason for concluding that, while the seas swarmed with trilobites and molluscs, the dry land, supplying the detritus amid which these remains were entombed, was not a desert waste, a mere mass of rocks decomposing under the action of the atmosphere, and worn away along the sea-level by the breakers; in fact, nothing but a storehouse for the production of the marine sediments of the time. We require a marine vegetation as a base for the existence of the sea animal life of the period; and we may fairly infer no lack of terrestrial vegetation flourishing beneath the atmosphere at the same time. What that vegetation may have been we have yet to learn; but as the range of the Silurian deposits becomes more known over the earth's surface, in regions where they have either never been covered by more modern deposits, or having been so covered, are now bared by denudation,—and every day we learn more and more of their distribution,—we may expect to obtain a better insight into the kind of plants existing at that remote geological period.

2. *Plants of the Anthracite Formation of Savoy.*—Among the labours of our Foreign Secretary, Mr Bunbury, during his late travels on the continent, was included an examination of the fossil plants from the anthracite formation of the Savoy Alps. The results of this investigation he communicated to us in a memoir, in which he not only describes the species of plants that came under his observation, but also gave us a history of the researches and opinions connected with the mode of occurrence of these plants, adding general views of his own.

As you are aware, M. Elie de Beaumont was the first, in 1828, to announce the fact, that near Petit Cœur in the Tarentaise, beds containing an abundance of plants, of the same species as those discovered in the coal-measures of the palæozoic period, alternated with other beds containing belemnites, and referred the whole to the period of the lias. The plants were determined by M. Adolphe Brongniart. Subsequently M. Elie de Beaumont published an account of beds occurring between Briançon and St Jeane de Maurienne, and included them in the same series. Plants obtained from these rocks were examined by M. Adolphe Brongniart, and identified by him with those of the coal-measures. From all the facts, M. Elie de Beaumont inferred that the beds with belemnites and ammonites, and those containing the plants, were parts of one whole, and that whole referable to the date of the lias and part of the oolitic series.

This announcement was startling to those who were accustomed to consider that the animal and vegetable life existing at each geological period had been so entirely swept away, and replaced by new species at another, that no species of one geological period would have its existence prolonged into another. The view of M. Elie de Beaumont was in consequence considered to require confirmation, and thus the subject remained, as Mr Bunbury has pointed out, until the meeting of the Geological Society of France, at Chambery, in 1844, when the observations of the members present led them to adopt the opinions of M. Elie de Beaumont.

When at Turin in 1848, Mr Bunbury carefully examined the fossil plants from the Tarentaise in the Museum. In this examination he experienced difficulties from the imperfect preservation of the plants, their confused mixture and distortion, and from the injury to the structure caused by their replacement by a coating of talc. The specimens in the Turin Museum afforded Mr Bunbury fourteen different forms (for he will not venture to call them species), of which nine are Ferns, two Calamites, and three Asterophyllites, or Annulariæ. "Two of these ferns," he observes, "*Odontopteris Brardii* and *Pecopteris cyathea*, may be pronounced, with tolerable certainty, to be identical with characteristic and well-known plants of the coal-measures. Three, or perhaps four, others have a strong resemblance to coal-measure plants, with which they may probably be specifically identical; but," he continues, "I cannot feel certain of them. Another seems to be a remarkable and hitherto unnoticed variety of *Odontopteris Brardii*, connecting that species with *O. obtusa* of Brongniart. The eighth is perhaps a new species, but its nearest allies are plants of the coal-formation. Of the ninth, the specimens are too imperfect to admit of determination. Of the remaining plants, *Calamites approximatus* and *Annularia longifolia* appear to be absolutely identical with coal-measure plants; and the other two, *Annulariæ* or *Asterophyllites*, are at least very similar to carboniferous forms. The other *Calamite* is undeterminable."

The occurrence of similar plants at the Col de Balme, and in the mountains above Servoz and Martigny, is then noticed, as also the absence of belemnites in beds interstratified with the others in those localities. The plants obtained by Mr Bunbury from the neighbourhood of Chamonix, and those seen by him in the Museum at Geneva, consisted of eight Ferns, one Calamite (species undeterminable) and one Asterophyllite. A well-preserved specimen of *Lepidodendron ornatissimum*, of Brongniart, was pointed out to him by M. Elie de Beaumont in the collection at the Ecole des Mines at Paris, brought from beds at the Col de Chardonet, near Briançon, eferred "to the uppermost part of the Alpine anthracite formation, and probably equivalent to the Oxford clay." It thus appears that the researches of Mr Bunbury lead him to conclude, with M. Adolphe Brongniart, that the plants from the beds noticed present a general agreement with those found in the coal-measures.

It will be fresh in your recollection, that the mixture or rather alternation of beds containing belemnites with others full of plants resembling those commonly found in our coal-measures, engaged the attention of my predecessor in this chair, Mr Horner, and that he pointed to the probability that it might be an instance of species which had a wide range in space having had also a long duration in time, calling your attention to the wide spread of similar plants over certain northern regions of our globe at apparently the same geological time. This explanation does not satisfy Mr Bunbury, inasmuch as other plants are known to be found elsewhere in European accumulations between the periods of the coal-measures and the oolitic series inclusive, admitting, however, that in the Permian system of Sir Roderick Murchison the character of the entombed plants closely resembles that of those of the coal-measures. He more particularly observes on the difference of the plants in the *grès bigarré* of Alsace, remarking on the common spread of certain ferns at the present day over Europe, and of the same tribe of plants over wide areas at the period of the coal-measures. He also points out the small geographical distance of localities in which the remains of these dissimilar plants are found in the rocks noticed, and calls attention to the observations of M. Scipion Gras, who states that the Jurassic rocks, occurring in their ordinary condition in the department of the Isère, contain impressions of plants entirely different from those of the Alpine anthracite. He admits, however, at the same time, that there are instances of the isolated occurrence of tropical plants, especially Ferns and Lycopodia, in temperate regions, far beyond their ordinary geographical range, as, for example, the growth of *Trichomanes radicans* in Ireland, and of *Lycopodium cernuum* in the Azores. Mr Bunbury then adverts to the hypothesis of M. Adolphe Brongniart, that the plants in question may have been drifted from regions in which the coal-measure plants still continued to grow,—in the same manner as seeds are now drifted from the tropical regions on the American side of the Atlantic to the shores of Europe,—in part, perhaps, becoming enveloped in deposits near land where plants similar to those producing such seeds do not occur. While he admits that this hypothesis is the most plausible under existing information, and that he has none more satisfactory to offer, Mr Bunbury does not see his way out of the difficulty.

3. *Fossil Land Plants, as illustrative of Geological Climate.*—Of all organic remains, perhaps those of land plants would appear to afford us the least direct information as to the climate, at different geological periods, of the low or slightly-elevated countries bordering seas in various parts of the world, except we can obtain something like evidence of the plants themselves having flourished so near the level of the seas of the time, that slight changes in that level produced alternations of deposits, which should at one time contain the remains of marine animals which inhabited the coast seas, and were quietly entombed, and at another the remains of plants, shewing

their growth on the spot. Such evidence we seem to possess at two distinct periods in the north of England, where we detect alternations of coal-beds, with their under clays, and limestones with marine animal remains of the carboniferous time; and also find a coal accumulation, with some plants, apparently in the position in which they grew, of the oolitic series. In both cases the evidence would be in favour of quiet depressions, low districts, with land plants growing upon them, so sinking beneath sea-water, that marine creatures swarmed over the previous dry land, their remains entombed amid detrital deposits effected at the time.

Viewing the actual and varied altitudes above the sea-level of lakes in different parts of the world, the plants which may be drifted into them and preserved amid any mud, sand, or calcareous matter deposited in such lakes, give us no just idea of the climate of the time, at the sea-level in the same latitudes. For instance, the plants drifted into the lakes of Switzerland and Northern Italy, some of which may even be swept from heights approaching lines of perpetual snow, would not give us the climate of the coast of the Bay of Biscay between the Saône and the Gironde, though in the same general latitude. Then, again, as to the conditions for the transport of plants or their parts to situations where portions of them may be more or less preserved in detrital matter, much has to be considered. Though floods in high regions tear up trees and smaller plants in their course, the chances of any of the plants reaching sea-coasts, depend upon a variety of conditions, among which proximity to the sea is one of no inconsiderable importance. Thus we have seen the arborescent ferns and other plants of the higher lands of Jamaica swept by floods into the adjoining seas (becoming entangled in part among the mangrove swamps at the mouths of the rivers), the distance having been so short, that many stems of the fern tree, their fronds, and those of other ferns of the higher regions, were not much injured. No mere swelling of the rivers from rains on the lower grounds, which did not cause torrents to wash away plants in the higher lands, would bring down a frond of these ferns; it would, however, sweep on many a lowland plant, and not a few of those which grew in the river courses during the dry weather, into the mangrove swamps and the sea.

In great rivers, the leaves, as they fall from trees overhanging the water, are floated onwards and often carried quietly to sea, sometimes from long distances inland. Plants and their parts may, under favourable conditions, be washed into, and be preserved in the mud of climates where they do not grow. They may be thus brought by the Mississippi, the Paraguay, the Nile, and the great rivers of Northern Asia flowing from south to north, and be preserved under climates differing from those where they flourished. We have no reason to suppose that the conditions of continents, as regards the flow of rivers into the sea, were not very various during long lapses of geological time; and we should very carefully avoid permitting our view of the relative disposition of land and water at former periods to be biased too far by their present arrangement.

Every autumn our European rivers are full of leaves which have quietly fallen into them. Some get washed on the banks, while others are left upon low grounds when the waters may have been more swollen at one time than another. Some get borne backwards and forwards by the tides in estuaries, and are accumulated in the mud, entangled with the remains of estuary animals and plants; but many get washed to sea, particularly if off-shore winds prevail at the time. Probably many of these become saturated with sea-water and fall to the bottom amid the remains of marine molluscs and other animals, and are thus entombed with them amid any detritus there accumulating. Some we know are thrown on shore, at various distances from the river-mouths, according to the prevalence of the winds at the time, and the relative bearing of these upon the coasts of the locality, and become intermingled with various marine animal and vegetable remains.

The extent to which trees and smaller plants are washed during floods out of the great rivers of the world, and floated outwards to situations where they fall within the influence of ocean currents and prevalent winds, is very considerable; and it is very needful to bear this in mind when we have no satisfactory evidence as to the growth of plants at or near the localities where we find their fossil remains. Little islets of matted plants are thus sometimes floated away, and it will depend upon the weather they may encounter how long they may keep together before they become broken up by the seas, and fall to the bottom. Although the counter-current along the Atlantic shore of the United States may tend to carry plants washed out from the rivers of that part of North America to the southward, the Gulf Stream is still enabled to transport plants and their parts from Cuba and the Bahamas (the prevalent trade-winds even perhaps drifting them from Hayti) northerly towards Newfoundland. Taking the Gulf Stream and its counter-current along the American shore as constants, we may have two north and south belts beneath, in one of which the remains of plants from the north are accumulated, and in the other those from the south, indicating climates which do not correspond with those of the dry land of America in the same latitudes. Such lines of transport—and there would appear to be many of them—and the probable falling of plants and their parts to the bottom during a long period of time, have to be regarded when we consider deposits wherein the remains of plants which may not have grown on the spot are entombed. There may be situations where little detrital matter now settles, but where drifted vegetable matter may accumulate from the repetition of certain annual effects continued through long time, as well as those deposits which we infer have been the result of the growth of plants on or near the spot where their remains are now found. When we consider all the conditions under which the remains of plants may be accumulated, and the difficulty often of determining the real character of the plants themselves, it would appear desirable to obtain more information respecting the distribution of fossil plants at different geological times than we now possess, before we conclude that we have evidence enough to speak of the characteristic plants of different geological epochs with the confidence sometimes used. It would appear very desirable, under present information, to regard the subject more locally, and always with reference to the probable physical conditions under which the plants may have been entombed.

4. *Co-existence of certain Saurian and Molluscous Forms at Equal Geological Times.*—To Professor Owen we are indebted for a description of saurian remains discovered by Professor Henry Rogers in a greensand deposit of the United States, considered referable to the age of part of the cretaceous accumulations of Europe. The specimens placed before Professor Owen enabled him to add some facts to the osteology of the Mosasaurus, and to discover some species of saurians, especially of the procœlian form of crocodile, not previously known in strata older than the tertiary deposits termed eocene. After very important osteological details respecting the Mosasaurus, which require to be studied in the memoir itself, in order fully to appreciate the labours of our colleague upon this subject, he states, that, considering certain of the bones to belong to the Mosasaurus, "they indicate the extremities of that great saurian to have been organised according to the type of the existing Lacertia, and not of the Enaliosauria or marine lizards;" and adds, "two species, at least, of true Lacertia have left their remains in our English chalk."

Professor Owen next notices some remains of a procœlian reptile, and proposes to indicate the saurian and probably mosasauroid genus to which it belongs by the name of Macrosaurus. Upon other remains he establishes the genus *Hyposaurus*, an Amphicœlian crocodile, and then notices specimens from the same localities laid before him by Professor Henry Rogers, which he remarks are "the first evidences of the genus of the modern Crocodilus or Alligator that have been discovered in strata older than the eocene tertiary."

The accumulations amid which these saurians have been detected are inferred, from the marine remains found in them, to be of the same age as part of the cretaceous series of Western Europe, similar marine molluscs having been considered to exist and to have been entombed in mineral matter at the same geological period in the seas surrounding the shores of land in the areas now occupied by the United States and Western Europe. The remains described by Professor Owen thus possess not only high interest, as additions to the forms of life which have existed at different times on our earth, but also as shewing the co-existence of certain saurian and molluscous forms at equal geological times. We have thus the modern crocodile or alligator (living probably much in the same way as the species of the same genus do in the present day, namely, in rivers and estuaries) borne into seas in which molluscs of the same kinds as have their remains entombed in our cretaceous rocks, were living.

From the general characters of the other saurians found we should also infer that their habits were not such as to render the sea among their usual haunts, but rather that they lived in rivers and estuaries, occasionally coming on the adjoining lands. When we look at the lithological characters of the beds in which these remains are entombed, as well as to the state in which the bones are preserved, it at once becomes evident that they have been carried to the situations at or near which they are now discovered, by being rendered specifically lighter than they now are, or formerly could have been. In fact we seem required to consider that flesh was on the bones when they were borne into the seas, amid the deposits and creatures living at the time, in which they are detected. However dificult it may be to wash crocodilian animals into the adjoining seas from out many of the great rivers of the world where these creatures live in multitudes, more particularly where mangrove swamps abound at their embouchures, this is not the case with the short torrent rivers descending from high lands into the seas surrounding islands, as, for instance, Jamaica and Hayti. During a great flood in the Yellahs river, one which takes its rise in the Blue Mountains of Jamaica, and at whose mouth and in the adjoining mangrove swamps the caimans are common, the body of water was so great as to sweep these crocodilians off to sea, where it may be presumed some perished, to leave their bones, at least such as were not swallowed by the large fish, to be mingled with the remains of marine molluscs now living in those seas. In cases of floods of this kind, the suddenness of which can be scarcely appreciated by those who have not witnessed the waters of heavy tropical rains discharged by means of a short steep course from high mountains into the sea, many a river and estuary air-breathing creature gets overpowered and carried off before it can reach the protection of eddies near the banks; and should there be a heavy sea going at the time, as sometimes happens when a hurricane is accompanied by floods of rain, there is a poor chance of their escape from drowning, however well fitted for living in rivers and estuaries under ordinary conditions.

5. *Phosphate of Lime in the Mineral Kingdom.*—The agricultural importance of phosphate of lime has of late years caused more search to be made for this substance than formerly, though its occurrence, as a component part of certain organic remains and of some rocks, has been long known. Mr Paine, of Farnham, having pointed out that certain beds contained phosphate of lime in sufficient abun-

ance to render them of much agricultural value, our colleague, Mr Austen, was induced to investigate the mode of occurrence of the phosphate of lime in his own neighbourhood, that of Guildford. He found that the phosphate of lime nodules are abundant in the upper greensand. They also occur in the gault, in two distinct beds, remarkably persistent, in the district.

Mr Austen regards the phosphoric acid of the nodules as of animal origin. When the nodules are rubbed down they present a concentric arrangement of parts, resembling bodies formed, like agates, by infiltration into cavities; and our colleague points out that, where the casts of bivalve shells and ammonites are filled with matter containing phosphate of lime, these forms must have been first inclosed in the sand, that then the proper shelly matter was removed, and finally that the earthy phosphate occupied the place of the hollow. He supposes that the phosphoric acid may have formed part of the coprolitic matter of the time, this matter in part preserved with its original external form, while more frequently it was broken up and the component portions diffused amid the sand and ooze. He also draws attention to the conditions to which the beds containing these substances have been exposed since their formation, having been covered by thick deposits, and having descended to depths beneath the level of the sea, where they were exposed to an elevated temperature corresponding with the depth and the amount of bad heat-conducting bodies above them, so that many chemical changes were effected, and among them a more general diffusion of phosphoric acid in the mass.

Mr Nesbit has also communicated to us some remarks on the presence of phosphoric acid in the subordinate members of the cretaceous series. He states that he mentioned to Mr Paine, in November 1847, the existence of a large amount of phosphoric acid in a fertile Farnham marl, and that he subsequently obtained 28 per cent. of phosphoric acid from portions of this marl, the general mass containing about 2 per cent. Nodules from the Maidstone gault also gave him 28 per cent. of phosphoric acid. Other localities are noticed, and as much as 69 per cent. of phosphoric acid is mentioned as contained in a dark red sandstone rock occurring in masses in the upper portion of the lower greensand at Hind Hill.

Mr Wiggins has sent us a notice of the fossil bones and corprolitic substances discovered in the crag of Suffolk, remarking on the value of the latter for agricultural purposes,—200 tons of them having been obtained from about a rood of ground; an additional instance of the remains of animals and their fæces, entombed in rocks of different geological ages, becoming available for the growth of existing plants.

As regards phosphate of lime and its dissemination, which modern researches have shewn is much greater, when sufficient quantities of rocks are examined, than appear from the analyses of the small portions usually employed,—a matter of interest when we consider the phosphate of lime required for certain plants,—we should recollect that when free carbonic acid is present in water, the phosphate, like carbonate of lime, though not to the same amount, is very soluble. Hence, especially when, as noticed by Mr Austen, phosphate of lime is disseminated in the state of fresh corprolites amid detrital matter, and water containing free carbonic acid is present and can have access to it, the phosphate of lime would be in a condition to be removed and disseminated. Mr Austen has alluded to the mixture of such bodies with vegetable matter, to the decomposition of which, with animal matter also, we might look for some, at least, of the carbonic acid that would aid the solution of the phosphate of lime. As in the case of the carbonate of lime previously noticed, when the solution of this phosphate met with the silicates of potash or soda, whilst percolating amid the rocks, the silicates would be decomposed by the carbonic acid, and the phosphate of lime thrown down. We should expect,—in the same manner as carbonate of lime often replaces the original matter of a shell which has been decomposed and removed from the body of a rock, leaving those cavities commonly termed casts,—that phosphate of lime, in localities where, from accidental circumstances, it was somewhat abundantly filtering through rocks, would also enter these and any other cavities, filling them under the needful conditions of deposit. In like manner as we find carbonate of lime separating itself from mud and silt in which it was disseminated, forming the nodules so common in calcareo-argillaceous deposits, should we also expect disseminated phosphates of lime to do the same under fitting conditions; so that it would not necessarily follow, however true in numerous cases, that nodules containing much phosphate of lime were coprolitic. We can readily imagine circumstances very favourable for the solution and spread of these phosphates amid layers of mud and silt. We find such phosphates surrounding some fossils, such as crustaceans from the London clay, leading us to infer a connection between the animal matter and this substance.

On a New Species of Manna from New South Wales. By THOMAS ANDERSON, M.D., F.R.S.E., Lecturer on Chemistry, and Chemist to the Highland and Agricultural Society of Scotland. Communicated by the Author.

The saccharine exudations of plants which have been classed under the generic term of Mannas, present, in all instances, a close resemblance in their chemical constitution. Their principal constituents are, gum, sugar, and the peculiar principle called mannite, which derives its name from its source, and has been considered as the characteristic constituent of a manna. All the varieties of manna obtained from European or Asiatic plants which have been examined contain this substance in greater or less abundance, and it appears also to be a common constituent of the fluid exudation of the leaves known by the name of Honey-dew. At least, this is certainly the case under certain circumstances, as it was observed by Langlois* in the honey-dew of the lime, which, during the hot summer of 1842, occurred in such abundance in the neighbourhood of Strasburg, that it fell from the trees in the form of small rain.

About 30 years since, a species of manna was brought to this country from New South Wales, which was obtained from the Eucalyptus mannifera, and differed in many of its properties from the European mannas. This substance was examined by Dr Thomas Thomson†, who ascertained it to contain a species of sugar resembling, and yet different from, mannite. It was afterwards examined by Professor Johnston‡ who confirmed Dr Thomson's observation, and by analysis obtained for this new species of sugar the formula $C_{12} H_{14} O_{14}$, which removes it altogether from mannite, and brings it into the class of the true sugars, containing hydrogen and oxygen in the proportion to form water, and further establishes its isomerism with grape-sugar, from which, however, it manifestly differs in all its properties. This was the first manna examined which contained no mannite; and I have now to add to the list another, similar in this respect, but differing in every other, and peculiarly remarkable from its possessing a regularly-organised structure.

The specimen subjected to analysis, I owe to the kindness of Mr Sheriff Cay, by whose son, Mr Robert Cay, the substance was originally discovered in the interior of Australia Felix, to the north and northwest of Melbourne. An immense tract of country in this district is entirely occupied by a "scrub," as it is called in Colonial language, consisting of the mallee plant, Eucalyptus dumosa, the leaves of which at certain seasons become covered with this species of manna, which is known to the natives by the name of Lerp, the *l* being pronounced like the Italian *gl*. This substance was first observed by Mr Cay in the latter part of the year 1844, when he explored a considerable district lying between lat. 36° 20′, and 37° 10′ S., and long. 142° 40′, and 144° 20′ E. in search of pasturage for sheep. He returned in 1845 to occupy the ground, and, in the course of his journey was obliged to leave his party, in pursuit of a native guide who had decamped with a gun. In mentioning this incident, Mr Cay writes (25th March 1845): "I was rather cold that night, as I had come off after him in my shirt-sleeves; moreover, I had no dinner, but I got plenty of lerp. Lerp is very sweet, and is formed by an insect on the leaves of gum-trees; in size and appearance like a flake of snow, it feels like matted wool, and tastes like the ice on a wedding-cake."

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* Journal für Practische Chimie, vol. xxix., p. 444.

† Organic Chemistry, Vegetables, p. 642.

‡ Journal für Practische Chimie, vol. xxix., p. 485.

On Mr Cay's arrival in Scotland in 1847, he gave some further particulars regarding this substance, stating that it was produced in great abundance, and covered large tracts of the scrub like snow; that it is very nutritive, the natives becoming fat during the season in which it is found, and that he himself had subsisted for a day or two upon it; that it adheres with very little tenacity to the leaves, and is immediately washed off by a shower of rain.

As it appeared from this description, that the substance was unknown in this country, Mr Cay, at his father's request, wrote to his overseer in Australia, who sent over the quantity of lerp which has formed the material for my observations, accompanied by a letter, dated 25th February 1848, of which the following is an extract:—"The Blacks say the lerp is not in any way produced by an insect, but that it is a spontaneous production of the mallee or gum-scrub when very young, say a foot or eighteen inches high, and that it grows on either side of the leaf; that old mallee or mallee about eighteen inches high, does not produce lerp.

Therefore, this year they have burned as much of the mallee as they could to admit of the young mallee springing up."

The only published notice of this substance I have met with, is contained in Westgarth's Australia Felix, page 73, where it is mentioned in the following terms:—"Mr Robinson, the chief protector (of the Aborigines), ascertained during his expedition, in 1845, to the north-west of Australia Felix, that the natives of the Wimmera prepare a luscious drink from the Laap, a sweet exudation from the mallee (*Eucalyptus dumosa.*) This liquor is manufactured in the months of February and March, on which occasions there is commonly a festival and adjustment of mutual disputes."

The substance to which these observations refer, differs very strikingly in its external appearance, from all the other species of manna. It consists of numerous small conical cups of the average diameter of one-sixth of an inch, with a more or less distinctly striated structure, and covered externally with a number of white hairs curled in various directions. These hairs are not distributed over the whole external surface of the cup, but are generally attached to the middle portion between its base and apex. The cup itself is generally sharply acuminated, and bears a pretty close resemblance to some of the smaller species of patella. Its interior is pretty smooth, its exterior rough, and its edge perfectly regular and round. The cup and hairs are translucent, except on the edge of the former, which is frequently opaque. No traces of attachment to the leaves of the plant were to be detected, and though fragments of leaves, obviously those of a species of Eucalyptus, were found in the substance, none of them had any of the cups attached to them. The cups were not generally isolated, but usually adhered loosely to one another by the edges, and this attachment was always such that the mouths of the cups were in one plane, and there can be little doubt that it was by this surface they were attached to the leaves. The hairs, when examined under the microscope, were found to be distinctly organized. Each hair formed a uniform tube, which, under a high magnifying power, presented a granular structure, with imperfect indications of transverse striæ. When treated with potash under the microscope, they became very transparent, and lost their granular appearance, and a drop of solution of iodine coloured them uniformly blue; thus indicating starch as one of their constituents. The cup itself is composed entirely of a mass of cells resembling starch-globules, but so closely compacted together, that their characters can only with difficulty be made out. A thin slice, however, when macerated for some time in water, admitted of disintegration, and though most of the cells were broken up, a few could be distinguished in a pretty perfect state, and agreed in their appearance with those of starch. The whole cup is coloured blue by iodine.

The taste of lerp is distinctly saccharine, but this is confined entirely to the hairs; the cup when completely separated presenting only a slight mucilaginous taste.

The chemical examination shewed that it differed as remarkably in constitution as it does in form, from all hitherto examined species of manna. When boiled with alcohol, a large proportion is dissolved; but the solution deposits no mannite on standing, and when evaporated on the water-bath, yields a thick syrup, which cannot be brought to crystallise. It is obvious, from this fact, that it contains neither mannite nor the sugar obtained by Johnston from the manna of Eucalyptus mannifera. The sugar separated from lerp had all the characters of the uncrystallisable sugar obtained from fruits, and entered rapidly into fermentation when mixed with yeast. The residue from which the sugar had been extracted yielded to cold water a small portion of gummy matter, and, when boiled with water, a considerable part of it dissolved, and the filtered solution, on cooling, deposited a large quantity of a white powder, of sparing solubility in cold water. The fluid from which this substance had separated gave, with iodine, a strong reaction of starch.

The substance which deposited from the hot solution, when washed with hot water until it no longer gave the reaction of starch, was found to agree, in all its characters, with inulin; but in order fully to establish its identity, an analysis was made of the substance dried at 310°, of which the following are the details:—

6·441 grains gave
10·398 … of carbonic acid, and
3·652 … of water,

giving the following results per cent.:—

Carbon, . . .	43·90
Hydrogen, . . .	6·29
Oxygen, . . .	49·81
	100·00

which agrees perfectly with the results obtained for inulin from other sources.

The insoluble residue was likewise carefully washed with boiling water, and then constituted a white substance insoluble in water, alcohol, acids, and alkalies, and agreeing in its characters with cellulose. That it actually was this substance, was determined by the following analysis of the substance at 212°:—

3·953 grains of cellulose gave
6·334 … of carbonic acid, and
2·494 … of water.

Carbon, . . .	43·69
Hydrogen, . . .	7·00
Oxygen, . . .	49·31
	100·00

Traces of nitrogen, and of a waxy or resinous matter, were also detected; but of these, and more especially of the former, the quantity was too minute to admit of determination. When burnt in the air, it left behind 1·13 per cent. of a white ash.

The quantitative analysis of lerp presented some difficulties. These were chiefly experienced in determining the quantity of starch, which I at first attempted to do in the usual manner, by washing it out; but the hairs disintegrated under pressure, and passed in fragments through the cloth, so that I was under the necessity of abandoning this process, and determining it by difference. This was effected in the following manner:—The residue, after extraction by alcohol and cold water, and which, of course, contained the starch, inulin, and cellulose, was weighed, and then boiled with water. The insoluble residue of this process, which was cellulose, was washed, dried, and weighed; the inulin which deposited from the boiling solution on cooling, was likewise washed, dried, and weighed. The difference between the sum of these weights, and that of the whole original residue, was reckoned as starch. This method, which was the best the circumstances admitted of, is not one of very high accuracy; but I believe it to approximate pretty closely to the truth. I think it likely, however, that the starch is rather under, and the inulin overrated, as, owing to the slight solubility of the latter substance, it was impossible to carry the washing very far. The following are the results I obtained:—

Water,	15·01
Sugar, with a little resinous matter,	49·06
Gum,	5·77
Starch,	4·29
Inulin,	13·80
Cellulose,	12·04
	100·00
Ash,	1·13

Such being the constitution of this curious substance, the question of its origin becomes of very great difficulty. All the species of manna regarding which we have explicit information appear to be exudations consequent upon the puncture of an insect, and they are composed of substances entirely soluble in water, which may easily be conceived to exude in solution, and gradually dry up in the rays of the sun, as indeed is actually the case with common commercial manna. But in this manna, we have present the insoluble cellulose, with starch, which is absolutely insoluble, and inulin, which is sparingly soluble in cold water; and it is very difficult, under any circumstances, to suppose that these substances could have been produced as a consequence of a puncture, and still more so, when it is taken into consideration, that the whole substance is possessed of a definite organisation. It is true that certain insect punctures are followed by the production of a sort of organised excrescence on some plants; but in every instance these are excrescences in the strictest sense in the word, and are part of the plant upon which they are developed, but lerp is manifestly an independent substance, the very attachment of which is not distinguishable; and I apprehend that far more distinct evidence than we now possess is required to establish its insect origin. The natives, as has been already mentioned, state that it is not produced by an insect, and though, under any other circumstances, the opinion of a tribe so unintelligent as the New Holland aborigines is not deserving of any attention, it is still of some importance when it tallies with the conclusion to which I think the chemical examination leads us. Entomologists to whom this substance has been shewn, are of a different opinion; and Mr Newport, to whom specimens were sent, has gone so far as to establish, on the strength of it, an entirely new genus of insects, to which he has given the name of Aspisarcus, from ασπις a shield, and αϱϰυς a net.* The consideration of this point, however, must be left to those who are more competent than I am to form an opinion. I have confined myself to determining its constitution, which appears to me altogether at variance with the idea of its being a simple exudation consequent upon the puncture of an insect.

Statistics of Nutmegs.

The statistics of nutmegs are very imperfect, but still we have sufficient data to enable us to form some estimate of the cultivation and production in the different parts of the Indian Archipelago, where the plant is cultivated. In the Straits

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* Professor Balfour, in his Manual of Botany, p. 412, says: "A saccharine substance, mixed with cellular hairs, which arise from a cup-like body, has been sent to this country by Mr Cay, found upon the leaves of *Eucalyptus dumosa*. It is called Layurp by the natives, and is thought, by Mr Newport, to be the produce of an insect of the tribe Coccidæ."—Ed.

Settlements the cultivation is extending very largely, and the production, of course, keeps pace with it. It was only in the beginning of the present century that nutmeg planting was introduced into Pinang, a number of spice plants having been imported from Amboyna by the East India Company.* The Government, after some time, sold their gardens in which they had planted the clove and nutmeg trees, but the cultivation would appear to have made little progress at first, as, in 1810, we find that there were only about 13,000 trees on the island, a few *hundreds* being all that were in bearing. In 1818, the number of bearing trees had increased to 6900. In 1843, there were 75,402 trees in bearing, and 111,289 not in bearing, besides males, and 52,510 in nurseries. The cultivation has been steadily increasing since that date, and the greater part of the trees then planted out, but not bearing, must now be yielding fruit. The number of bearing trees in Province Wellesley, in 1843, was 10,500, not bearing 7307, besides males, and a number in the nursery. The total number of nuts produced by the Pinang and Province Wellesley trees, in 1842, were 18,560,281, and 42,866 lb. of mace.

Nutmeg trees were first introduced into Singapore in 1818. In 1843, the total number of trees were estimated at 43,344, of which 5317 were in bearing, the produce being stated at 842,328 nuts. In 1848, according to the table given by Dr Oxley,† the total number of trees planted out was estimated at 55,925, of which, the numbers in bearing were 14,914, and the produce 4,085,361 nuts, besides mace, which is estimated about 1 lb. for every 433 nutmegs. In Singapore, the cultivation is extending very rapidly. The increase does not take place gradually, but every now and then. When some person with capital enters upon it, it seems to receive a large impetus, the example set by one appearing to incite others to embark in it. In one district in Singapore this has been very apparent. The district of Tanglin, in the beginning of 1843, consisted of barren-looking hills, covered with short brushwood and lalang, which had sprung up in deserted Gambia plantations. Immediately upon the regulations for granting lands in perpetuity being promulgated in the middle of that year, a great part of the district was cleared, and nutmeg plantations formed, and there cannot now be less than 10,000 trees planted out in it. A number of Chinese are at present forming plantations in different parts of the island; one Chinaman has commenced planting, which he intends doing to the extent of 5000 trees, and we are aware of various individuals who propose to form plantations of greater or less extent.

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* Low's Dissertation on Pinang and Province Wellesley.

† Journal of the Indian Archipelago for October 1848.

During the occupation of Bencoolen by the English, the nutmeg and clove were introduced from the Moluccas, and in 1819, the number of nutmeg trees were stated at 109,429. Regarding their present number we have no information.

The spice trade of the Molucca islands being a strict monopoly, very few particulars are known regarding the extent of the cultivation, or the amount of the produce. The average quantity of nutmegs annually sold by the Dutch East India Company in Europe, during the last century, has been estimated at 250,000 lb., besides about 100,000 lb. sold in India. Of mace, the average quantity sold in Europe was reckoned at 90,000 lb. per annum, and 10,000 lb. in India. The trade, although so jealously guarded by the Dutch, has never been a very profitable one to them, the expenses being heavy. The large quantities of spices frequently burned in Holland, on which heavy charges for freight, &c., must have been incurred, must have also formed a serious deduction from the gross profit from those sold.* In 1814, when in possession of the English, the number of nutmeg trees planted out were estimated at 570,500, of which, 480,000 were in bearing, including 65,000 mocecious trees. The produce of the Moluccas has been reckoned at from 600,000 to 700,000 lb. per annum, of which one-half goes to Europe, and about one-fourth that quantity of mace. The imports into Java, from the Eastern Archipelago in 1843, consisted of nutmegs 740,033 piculs. and of mace 218,006 piculs, and the exports consisted of nutmegs 2,133,029 piculs, and of mace 486,063 piculs. The amount of nutmegs exported from Java, during the ten years ending in 1834, averaged yearly about 352,226 lb., and, during the eleven years ending 1845, about 664,060 lb. yearly. The quantity of mace exported, during the first period, averaged 94,304 lb. yearly, and during the last 169,460 lb. yearly.

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* Stavorinus' Voyages.

The average yearly consumption of nutmegs and mace in Great Britain is estimated at about 140,000 lb. The produce of the Straits settlements in 1842, was reckoned at, nutmegs 147,034 lb., and mace 44,822 lb., thus being more than equal to the whole consumption of Great Britain. The rest of Europe, it has been estimated, takes about 280,000 lb. of nutmegs, and 33,000 lb. of mace; India about 216,000 lb. of nutmegs, and 30,000 lb. of mace; and China about 15,000 lb. of nutmegs, and about 2000 lb. of mace. As these quantities, however, would leave a surplus production of nutmegs alone above 250,000 lb., it is probable they are now considerably under the real amounts. In ten years, from 1832 to 1842, the exports of nutmegs and mace from Pinang were trebled, and from the very great extension in the cultivation which is constantly going on, it is probable that the same result at least, will take place in the ten years succeeding to the above period, viz., from 1842 to 1852. During these ten years, from 1832 to 1842, the price of nutmegs in Pinang fell from ten and twelve dollars per 1000, to from four to five dollars per 1000. They have since kept at the latter rate, owing no doubt to the means taken by the Dutch, who at present regulate the market, to maintain the price; but it must be no less evident, that, with the large accumulations which this occasions, and the enormous increase in the production, the price must sooner or later give way, as it has done before, and go down permanently to a considerably lower rate. If a decrease takes place at longer or shorter intervals, notwithstanding all the pains used by the Dutch to keep up the market, what would be the result were the spice monopoly abolished, and the trade and cultivation rendered free and unrestricted? There would, without any extension of the cultivation in the Moluccas, but merely from greater care and skill being applied by the persons who would probably embark in it, be a very considerable increase in the production from the present plantations. The produce being sent at once into the market, in increased quantities, to be sold for what it would bring (for private cultivators or merchants could not afford to hold back and regulate the quantity like the Government), a very serious fall would inevitably result, which would no doubt be permanent and steady; because, as regards nutmegs, it may be safely stated that the supply already exceeds the demand, and that any increase in the supply can only be got off by submitting to a reduction in price. That we may not be suspected of exaggerating in regard to the Moluccan plantations, we refer the reader to Count Hogendorp's Account of them, and of the wretched management to which they were subjected at the time when he wrote, and which prevails at the present moment. Throwing them open to private enterprise, could not but have the effect of improving and probably extending the cultivation to a large extent, and of course causing a very large increase in the production. The Dutch Government at present derive little or no profit from the monopoly, so that it is very likely it will be soon abolished, in compliance with the demand which is now made in Holland, as well as in the colonies, for a more liberal system of trade; and there is no doubt that the giving it up would be a popular measure. Already, the influence of free trade has penetrated into that so long jealously-guarded region, and the making Menado and Kima, which are under the Molucca Government, free ports, may only be the prelude to opening the Spice Islands themselves to the general trade, a measure which, of course, would entail along with it the necessity of abolishing the monopoly of spices.

It may appear that we have written rather discouragingly regarding nutmeg planting, and that the picture we have drawn of it is as much too sombre as that of Dr Oxley was too bright and glowing. We have, however, only given such facts and information as we could collect: from these we leave others to draw their own conclusions. It is probable that persons who have plantations already at maturity, or who, having capital, can afford to form their plantations with rapidity, and by high culture force the production, may still, for a considerable time to come, find nutmeg cultivation a source of profit, but to those who embark in it with but limited means, and can only extend their cultivation by gradual and slow degrees, it will certainly, in our opinion, prove a hazardous speculation, and one which prudence would seem to counsel them to avoid. Above all, to those who, like the Chinese, in their nutmeg planting in general, cultivate imperfectly, and, therefore, to a certain extent with less profit, it must in the long run leave anything but a satisfactory result.—(*Journal of the Indian Archipelago*, vol. iii., No. 1, p. 3.)

Account of a Craniological Collection, with remarks on the Classification of some Families of the Human Race. By Dr SAMUEL G. MORTON.*

PHILADELPHIA, *December* 1, 1846.

MY DEAR SIR,—I have great pleasure in giving you the information requested in your last letter; and, in so doing, shall endeavour to be as brief as possible.

Having had occasion, in the summer of 1830, to deliver an introductory lecture to a course of anatomy, I chose for my subject, "The different forms of the skull, as exhibited in the five races of men." Strange to say, I could neither buy nor borrow a cranium of each of these races; and I finished my discourse without shewing either the Mongolian or the Malay.

Forcibly impressed with this great deficiency in a most important branch of science, I at once resolved to make a collection for myself; and now, after a lapse of sixteen years, I have deposited in the Academy of Natural Sciences a series, embracing upwards of 700 human crania, and an equal number of the inferior animals.

The human skulls are derived from all the five great races, Caucasian, Mongolian, Malay, American, and Negro, and from many different tribes and nations of each.

A primary object with me had been to compare the osteological conformation of our aboriginal tribes with each other, and also with the other races of men; and, in pursuit of this inquiry, I have accumulated upwards of 400 American crania, pertaining to tribes placed at the remotest geographical distances, and subjected to almost every vicissitude of climate and locality of which this continent affords examples. I have already, in my *Crania Americana*, given the result of my observations; and I shall now repeat them with the greatest possible brevity.

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* The following letter from Dr Morton is in reply to a request made to him by Mr John R. Bartlet, secretary of the American Ethnological Society, for an account of his craniological collection, with a view to incorporate it in his "Progress of Ethnology." It was, however, found to be of so interesting a nature, that the Society determined to present it entire in the second volume of its Transactions.

. The anatomical facts, considered in conjunction with every other species of evidence to which I have had access, lead me to regard all the American nations, excepting the Esquimaux, as people of one great race or group. From Cape Horn to Canada, from ocean to ocean, they present a common type of physical organisation, and a not less remarkable similarity of moral and mental endowments, which appear to isolate them from the rest of mankind; and we have yet to discover the unequivocal links that connect them with the people of the Old World.

Both Europeans and Asiatics may, in former times, have visited this continent by accident or design. That the Northmen did so, is matter of history. The Phenicians, Welsh, and Gauls, may possibly have done the same thing. They may have had some influence on the language and institutions of the country, and modified and extended its civilization. But, granting all this (for the entire evidence is wanting), where are now these intrusive strangers? We answer, that if they ever inhabited this continent, they have long since been swallowed up in the waves of a vast indigenous population, which, in its present physical characteristics, preserves no trace of exotic intermixture. The Indian, in all his numberless localities, is the same exterior man, and unlike the being of any other race. His multitudinous tribes are not only linked by a common physiognomy and complexion, and by the same moral and mental attributes, but also, as the learned and justly distinguished Mr Gallatin has shewn,* by the structure of their languages, and by their archæological remains. The latter (wherever we find them) present evidences of the same constructive talent, varying only in the degree or extent of its development. It is seen on the grand and imposing scale in Yucatan and Palenque, and in the sepulchral islands of Titicaca; and it is not less obvious in those humbler efforts that are everywhere scattered over the great valley of the Mississippi. Open the mounds, as Dr Davis, Mr Squier, and Dr Dickeson have so laboriously and successfully done; and the very same arts and inventions, though in a mere rudimentary state, everywhere meet the eye. All point to one vast and singularly homogeneous race.

But it is necessary to explain what is here meant by the word *race.* I do not use it to imply that all its divisions are derived from a single pair; on the contrary, I believe that they have originated from several, perhaps even from many pairs which were adopted from the beginning, to the varied localities they were designed to occupy; and the Fuegians, less migratory than the cognate tribes, will serve to illustrate this idea. In other words, I regard the American nations as the true *Antochthones*, the primeval inhabitants of this vast continent; and when I speak of their being of one race, or of one origin, I allude only to their indigenous relation to each other, as shewn in all those attributes of mind and body which have been so amply illustrated by modern ethnography.

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* Mr Gallatin includes the Esquimaux dialect in this great family of languages. Further investigations may prove them to be an element of the great American race; but I *confess my own materials for this investigation have hitherto been altogether inadequate.*

But to return to my collection of skulls. It also contains the embalmed heads of upwards of 130 ancient Egyptians, taken from the tombs of Memphis, Thebes, Abidos, &c. These unexampled materials, for which I am chiefly indebted to the kindness and zeal of my friend Mr George R. Gleddon, have enabled me to prove, I believe, incontestably, that the Egyptains had no national affiliation with the Negro race. Their cranial characteristics can be distinguished at a glance; and the two nations, who are constantly represented side by side on the pictorial monuments of the Nile, are as different from each other as the White man and the Negro of the present day; and yet these contrasts look back to a period of time little short of 5000 years from the present day.*

My later investigations have confirmed me in the opinion, that the valley of the Nile was inhabited by an indigenous race before the invasion of the Hamitic and other Asiatic nations; and that this primeval people, who occupied the whole of Northern Africa, bore much the same relation to the Berber or Berabra tribes of Nubia, that the Saracens of the middle ages bore to their wandering and untutored, yet cognate brethren, the Bedouins of the desert.

Egypt, during the historical period, bears ample evidence of an Asiatic civilization engrafted on the rudimentary arts of the primeval inhabitants of the valley of the Nile; at the same time that our present knowledge, vastly augmented as it has been of late years, does not yet enable us to decide how much to ascribe to the conquering and how much to the conquered nation.

But with respect to the ancient Egyptians themselves, the denizens of the soil during the Pharaonic dynasties, how completely are they everywhere identified, on the monuments and in their tombs, as a people of peculiar national physiognomy, which mingles the Japetic conformation, on the one hand, with the Semetic on the other; thus placing them, in the ethnographic scale, intermediate between the two!

While, however, the pure Egyptian of the monuments is everywhere identified at a glance, those same monuments and the associated tombs, enable us also to detect the various exotic races with whom the Egyptians had intercourse in war or in peace. Among these are seen the people of Pelasgic origin, whose enbalmed bodies are so frequent in Memphis, and whose great number is accounted for by the long period of Ptolemaic rule;—the Semitic nations, as seen in the Hebrew and Arab cast of features;—the Scythians, who are always stigmatised as enemies, and branded with a curse;—the Negroes, who are represented on the monuments as slaves and captives, and share the same anathema as the Scythians; and lastly, without enumerating the many subordinate subdivisions of the human race, the Negroid population, which seems to have been numerous and well protected. These Negroid inhabitants are obviously a mixed race between the Egyptian and Negro (or rather negress), in which the features of the latter are in preponderance. I have a considerable number of their heads from the catacombs, especially of Thebes. It will be inquired, if Negroes were so much despised in Egypt, if they were in the position of slaves or bondsmen, how does it happen that their embalmed remains are of so frequent occurrence in the catacombs? This question is answered by a passage in Diodorus, wherein the historian informs us that every child whose father was an Egyptian, was from that circumstance free, and enjoyed the privileges of citizenship even when the mother was a slave.

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* See Böckh, Bunsen, Henry, &c.

But to revert again to the collection of skulls, from which I have been able to derive so many interesting facts, I shall merely add that it contains a fine series of the more distant Caucasian nations, Circassians, Armenians, Arabs, Persians, and Hindoos, with a smaller but characteristic group of Malays, Chinese, Polynesians, and Australians. Yet this large collection does not yet contain a single Esquimaux or Fuegian head! The extremes of this continent are not represented.

Pray make such use of this communication as your studies may suggest, and believe me, dear Sir, very sincerely yours,

SAMUEL GEORGE MORTON.*

J. R. BARTLETT, Esq.

A Description of several extraordinary Displays of the Aurora Borealis, as observed at Prestwich,† *during the winter of* 1848–1849; *with Theoretical Remarks.* By WILLIAM STURGEON, Lecturer on Natural and Experimental Philosophy, formerly Lecturer at the Honourable East India Company's Military Academy, Addiscombe, and late Editor of the "Annals of Electricity," &c.‡ Communicated by the Author.

Having had opportunities of observing several fine displays of the Aurora Borealis since the commencement of last autumn, some of which presented phenomena of very rare occurrence, a description of them, as they appeared at this place, can hardly fail to be interesting to philosophical inquirers, more especially as data are still wanting to establish a foundation for a true theory of the meteor, a physical problem of long standing, and, hitherto, without any satisfactory solution.

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* Transactions of the American Ethnological Society, vol. ii., p. 217.

† Prestwich is a village at the distance of four miles from Manchester, in a north-west direction, on the new road to Bury, from which it is also four miles distant.

‡ Read at the *Royal Institution*, Manchester, March 28, 1849.

The first grand display of the aurora borealis, in this list, occurred on Wednesday evening, 18th October 1848. It began with the close of the day, and lasted, with various degrees of brilliancy, till ten o'clock, or probably later; for, labouring under the effects of a severe cold, I could not watch it closely out of doors. It consisted of an extensive arch of light, which crossed the magnetic meridian at nearly right angles (which, however, was not its invariable position, but that which it assumed during the greater part of the display), and immense floods of lambent streamers, which occasionally flowed gently upwards and downwards, from various parts of the arch. The average colour of the light was that of a candle-flame, though in some parts, and especially towards the eastern extremity, the colour was red, inclining to violet. I observed nothing extraordinary in these streamers, nor in the general aspect of the aurora; but for reasons already stated I could not make a minute survey.

For several days previous to this aurora, the atmosphere had been highly charged with the electric fluid. On the preceding Saturday, I had the electrical kite elevated about 400 yards, from the string of which a small jar was rapidly and frequently charged; a steel needle was magnetized, and its poles reversed several times, by the discharge of the jar, and also by sparks direct from the kite-string. The magnetic polarity of the needle indicated a *downward* current in the string, which was the case in other experiments on several days previously, though not to the same extent of power.

This aurora was observed at many places wide apart, which shewed that it occupied an immense space in the heavens. It has been differently described by different observers, to whom it appears to have presented different aspects. The brief description given above, is copied from my journal, the particulars being written down on slips of paper as the phenomena occurred, and afterwards copied into the journal, which is my usual mode; for it is next to impossible to remember all the varied features which the meteor presents during the several hours that is sometimes required to watch its manifold and rapid transformations.

The next display of the aurora borealis, of any consequence, occurred 27th October. During the morning and all the forenoon we had continuous rain, which cleared off about two P.M. I had been looking out for the aurora all the evening, and about six o'clock an arch of dim light appeared in the northern heavens. It was very low, and not of that extensive horizontal span occupied by the aurora *of the 18th instant.* The western extremity reached a little westward of *Arcturus;* the star being much higher than the auroral arch, but as it was fast descending towards the horizon, it passed through the arch, whilst the latter remained stationary, or nearly so. Some fine groups of lambent streamers occasionally flowed upwards from different parts of the glowing bow; and also another feature which the aurora sometimes displays—the gentle blushes of pale soft light, were frequently seen in the dim haze that almost invariably accompanies the aurora borealis. Between seven and eight o'clock, a few straggling clouds came floating across the aurora. Such interruptions to the observer's view are exceedingly interesting events, for they never fail to shew that the auroral light is at a greater distance from the place of observation than the clouds themselves, which is one step, at least, gained towards obtaining a true theory of the cause of the meteor; but should nothing farther be ascertained, by the interposition of these clouds, than the locality or region of the atmosphere in which the aurora is situated, it would be the means of setting at rest an inquiry of great interest, concerning the real height of the meteor.

There are other features occasionally conspicuous in the aurora borealis, which have long been noticed, and rendered as the most astonishing appearance of the whole, I allude to the colours that sometimes adorn the meteor. They have for a long time appeared to me to arise from a decomposition of the true auroral light (white light, or rather that of a soft, pale candle-flame), accomplished by refractions and reflections amongst the abundance of aqueous particles hanging in the regions of air, where the electric fluid is in motion, or between those regions and the eye of the spectator. There can be no doubt of the electric origin of the aurora borealis, since many of its characteristics can be beautifully imitated by the electrical apparatus. The violet tint is easily produced by an electrical discharge through highly-attenuated air; but the green, the blue, the orange, the yellow, and the deep red, cannot be imitated by any form of electrical experiment hitherto known, in which the light is shewn in common air, however much it may be attenuated. But these colours may easily be accounted for, under the supposition of an abundance of aqueous vapour in the regions of an auroral display, a concession by no means unreasonable, when we take into account the season of the year (from about the autumnal to the vernal equinox), in which such spectacles are most frequent, the hazy appearance of the sky at the time, and the occurrence of wet weather that usually follows.

By looking over my journal for several years past, I find that the grandest displays of the aurora borealis have been closely followed by wet weather; and the following extract from the description of an aurora which I observed in the vicinity of London, on the evening of 3d September 1839, will probably appear more eminently calculated to develop the true character of the spectral colours accompanying the aurora borealis than any other attempt at explanation hitherto on record. The observations were made whilst walking from Brixton to my residence in Pomeroy Street, Old Kent Road.

"The sky was partially covered with thin vapoury clouds, which had an obvious influence on the colour, and the apparent horizontal motion of the light, which was easily discerned to be behind or beyond these thin clouds of vapour, and assumed a deeper tinge of redness as the vapour became more dense between it and the spectator. As this was the first time of my observing a red light during the display of an aurora borealis, I became anxious to know the cause, for I never saw the electrical light, in artificially-attenuated air, any thing like the colour of the light which I observed on this occasion. It was sometimes of a deep crimson, at other times of an almost fiery red; then pink, very light pink; next the yellowish-white colour which the aurora most usually displays; and so on for several alternate successions. At other times the aurora would seem to reverse the order of colours, beginning with the white light, and passing through the different red tints down to a perfect crimson, and then return gradually to the ordinary white. I had several opportunities of observing the curious changes of colour in the auroral light before I arrived at Camberwell. Just before I entered the Grove, at Camberwell, then about half-past nine o'clock, the northern sky was illuminated, throughout an immense horizontal range, with a rich red light; but when I arrived at the churchyard, about five minutes afterwards, the red light had nearly disappeared, a small portion only remaining on the northern edge of a thin fleece of vapour, at a considerable height above the western horizon, being succeeded by several fine groups of the usual white streamers."*

On this occasion I had an excellent opportunity of observing that the red light never appeared when the sky was pretty clear of those thin vapoury clouds; which frequently skimmed across the aurora and I eventually became so perfectly convinced of the effects they produced on the colour of the light, that I could predict the appearance of the red colour by observing the approach of the thin fleeces of vapour, before coming within the limits of the aurora. From these facts, it would appear that the prismatic colours which occasionally adorn the aurora borealis, are *secondary* phenomena, produced by the ordinary decomposition of the original light, and need not be looked upon as any thing extraordinary beyond the certainty of an abundance of aqueous vapour, either in the region of the electrical disturbance, or at a lower altitude in the atmosphere.

But to return to the aurora of the 27th October. The luminous arch, by admeasurement, never exceeded 12° of altitude; its highest point being close upon the magnetic north of this place. Its horizontal span was about 105°, but, in consequence of the diffused character of the light constituting this principal feature of the aurora, these dimensions can only be considered as close approximations to the truth. Before nine o'clock, dense clouds shrouded the aurora from view, and as the sky soon became covered with clouds, the spectacle closed for the night. I had a magnetic needle delicately suspended by a single fibre from the cocoon of the silkworm, which was closely watched, at every opportunity, during the whole time; about half-past seven it became slightly agitated, but made not any excursion either eastward or westward; its motion being a mere nodding in a vertical plane, which was continued for some time, probably much longer than the cause continued to operate upon it, as is always the case when needles are thus delicately suspended.

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* Annals of Electricity, &c., vol. iv., p. 403.

The next grand appearance of the aurora borealis was on Friday night, 17th November. A strong south wind with heavy rain began about six in the morning, and lasted till ten in the forenoon, about which time the wind veered westward until it arrived at north-west; and the rain cleared away. The wind still continued high; the thermometer was 50°. Slight showers towards evening, and the night was introduced by the most extensive aurora borealis I had ever beheld.

It began with the close of day, and lasted all night. I observed till twelve o'clock, at which hour, though cloudy, and scarce a speck of clear sky to be seen, the whole canopy was illuminated by the aurora; and the light generally, even in the southern regions of the heavens, was much stronger than that afforded by a *thinly*-clouded full moon.

I was in Manchester at the time of its commencement, and had no opportunity of seeing it till after my arrival at Prestwich, by the omnibus, about a quarter before eight o'clock. At that moment my attention was attracted by the unusual glare of light, and on looking up, I perceived immense floods of streamers flowing in almost every direction, and on every side excepting the south, which appeared totally devoid of them, though strongly illuminated. The east and west parts of the heavens appeared the most luminous of the whole, although all around the north was in a blaze of hazy streamers; indeed, every part of the aurora appeared as if composed of illuminated aqueous vapour, distributed in various forms in different parts of the circumambient aërial space. From the east, round by the north, to the west, these streamers appeared to flow *upwards*, in the usual way; but in the zenith, all about that point, they arranged themselves directly across the meridian; and on both rings of this line of streamers, and *southwards*, the auroral light consisted of fine, steady blushes, without any attendant streamers whatever.

Light fleecy clouds were passing over, with a brisk NW. wind at the time, and some rain had fallen whilst I was in the omnibus; but every streak and other form of cloud, all of which were exceedingly thin or attenuated, seemed to take a part in the auroral display; and those parts of the heavens which were not covered with cloud, appeared as if full of a luminous mist or haze, through which some of the principal stars were seen.

Such was the state of the aurora borealis when I arrived at home, then about a quarter before eight o'clock; but I was soon made to understand that I had lost the grandest part of the spectacle, which, as I was told, occurred about seven o'clock. The aurora had been watched by my family from about six in the evening, at which time the streamers were very fine; they occupied an extensive lateral range, and were of the usual pale colour. A little before seven, the streamers became more abundant, intensely brilliant, and reached over the zenith southwards, to the distance of twenty or more degrees; and, what increased the splendour of the scene, was a brilliant crimson canopy in the heavens, which became gradually transcoloured into a lively purple. On the eastern side, also, about 15° above the horizon, was an immense blush of red light, which gradually faded away and was lost.

These were the principal features of the aurora till about seven o'clock, after which hour its appearance was nearly the same as when I first saw it. Shortly after eight, an abundance of detached clouds floated over this locality, and partly obliterated the splendour of the meteor, which was now only occasionally exhibited in the openings amongst them. The wind being brisk, the groups of clouds that passed over made a quick transit, and soon gave place to a full display of the auroral glare, which, though strongest about the northern heavens, spread more or less over every part of the celestial vault.

Before nine o'clock the sky was again completely covered with thin clouds, but still a strong light passed through them, which gave a distinctness to objects, and to boundaries of land, as though it had been the twilight of a fine evening. About ten, an extensive blush of red light hovered in the southern parts of the heavens, at an altitude of about 40°, and continued nearly stationary for several minutes, with every appearance of the usual aërial spectra of an intense conflagration below. The curtain now dropped till nearly eleven, when an intense light, in the east and west, with a few streamers in the north, burst into view as if by magic; for thin clouds still obscured the stars except at occasional openings, where they were seen as bright spangles behind a luminous mist.

In one of these openings, an extensive blush of fiery red light appeared in the west, and gradually floated, southwards, along with the group of clouds that surrounded it, until it reached a little eastward of the southern meridian, where it appeared to remain stationary for a short time, gradually diminishing in intensity and dimensions till it finally disappeared. From this time till twelve o'clock, nothing remarkable was observed beyond a strong glare of light, which pierced the clouds and illuminated the whole expanse of country beneath.

The Seven Stars and Aldebaran were immersed in a strong auroral light at midnight, when the clouds had partly cleared away. The luminous bow or arch, which frequently attends the aurora borealis, never appeared during any part of the scene.

An aurora borealis appeared on the 18th of November, which consisted of a glare of light in the north, attended by a few shooting streamers.

Tuesday, November 21.—The morning was gloomy, with a light west wind. The thermometer stood at 40°, but rose to 44° in the forenoon. The afternoon was cloudy, with light showers of rain, and a brisk west wind. The sun set very red, and gave a glowing redness to the vicinal clouds, which were arranged in bands, in the direction of the wind from west to east; and shortly afterwards traces of an aurora borealis were depicted in the heavens.

The usual auroral bow appeared in the north before six o'clock, and, at a quarter past six, bands of streamers appeared in various parts of the heavens; and, at the same time, a broad field of red light hovered, for about two minutes, in the north-east. The bow, at this time, was not so high as the lower pointer in the Great Bear. At half-past six the arch or bow reached to an altitude of about 15°, but was very imperfectly formed; it became more and more diffused, spreading its light, in a short time, till it nearly covered the whole of the Great Bear. A few short streamers appeared at a low altitude in the north, as if they proceeded from a thin streak of cloud which crossed the meridian beneath, and parallel to the luminous arch.

About seven o'clock, several bands of dim red light started from the west, passed through the zenith, through Cassiopiæ, and extended to the eastern horizon; they were obviously illuminated streaks or bands of vapour, arranged by the westerly wind. There was scarcely any auroral light in the north at the time, and the stars looked very dim. At a quarter past seven a broad band of dim redness formed from west to east; it passed through the zenith and Cassiopiæ, and travelled slowly southward, until its eastern limb passed over the *Pleiades*. It broke up within three minutes after its formation; the eastern part quite vanished, but the western limb continued visible for a much longer time; it was always broadest and brightest on the western side. This phenomenon was immediately succeeded by a band of dim whitish light, which stretched across the northern heavens, having a horizontal span of 120°, and an altitude reaching that of the Great Bear. About this time, the whole of the northern parts of the concave seemed to be filled with broad dim bands of a smoky reddish colour, which had every appearance of being illuminated bands of vapour, arranged by the wind, at this time very feeble.

Several streaks of white light, at a higher altitude, but parallel to the former, were also observed, all of which were arranged in the direction of the wind.

At about ten minutes before eight, a number of flashes of dim white light shot across the heavens in various directions, and lighted up the streaks of vapour as they came at them in succession. At eight o'clock the thermometer had fallen to 42°, and there was a dead calm. At half-past eight several broad patches of feeble white light passed swiftly along the sky, in almost every direction; these were succeeded by a dim luminous white haze that seemed to fill all the northern heavens up to the zenith; it gradually waned, and, in a short time, finally disappeared, closing the scene for the night.

Sunday, December 17.—Fine frosty morning, with some fog. The day turned out fine, and the thermometer rose to 36°. At night we had a grand display of the aurora borealis. It began before seven o'clock, and lasted till after midnight. At half past seven the aurora consisted of three large patches of light, one in the east, one in the west, and the other just beneath the north star. Streamers shot from all these occasionally, but the principal light was in the west. About a quarter before eight an extensive range of streamers burst out all round the northern heavens, from the west to the east points, some of which assumed a dingy red hue for a moment, and then changed again to the soft white. There were two ranges of these streamers, one considerably higher than the other, with an unilluminated vacant space between them, though at first view the whole seemed to be but one group. The streamers of the uppermost range reached the altitude of Cassiopiæ, at that time a little westward of the meridian. Between eight and nine o'clock, several flashes or waves of light swept across the zenith and many other parts of the sky, in different directions; several of these waves proceeded from west to east, many from north to south, some of which reached the Seven Stars, and others traversed the heavens oblique to the former—all denoting an electric disturbance in the higher regions of the air, illuminating the highly-attenuated vapour in which it took place. These flashes or waves were of precisely the same character as those which appeared on the 21st of November, but their transit was much slower, so that the eye could follow them in their progress to their apparent destination. During this period of the aurora, several light rain-clouds scudded across from south to north, at a low altitude, and obscured the waves of light, evincing, as on other occasions, that the electrical meteor was above the clouds.

About nine o'clock, the whole of the celestial concave partook of the auroral scene, which exhibited very different aspects in different quarters. The northern parts had now become the brightest, though continually changing in intensity and tint of colour; the latter varying between a soft yellowish-white and a dingy red. In the south there appeared nothing but a lurid red haze, which gave a dimness to the stars, though some of them occasionally shone without any perceptible interruption. At one time Orion appeared as if completely covered with a flimsy mantle of a deep red colour; so flimsy, indeed, that the principal stars, Beteigeuse, Bellatrix, and Rigel, suffered but little from their usual splendour; the natural red tint of the former, however, was obviously enhanced by the auroral haze, and the others slightly partook of the flimsy red tinge; indeed, the whole of the stars in the southern heavens were more or less dimmed, and many of the smaller ones completely obscured. In the north, also, and, indeed, on every side, a thin haze prevailed in obstructing the natural refulgence of the stars, rendering them dim and gloomy. There was a brisk south wind all the evening, and the thermometer stood at about 34°. The whole display of the meteor on this occasion, and also on the 18th of November, appeared to take place in an atmosphere of highly-attenuated nubiferous matter.

1849. *Sunday, January* 14.—Stormy morning of west wind and heavy rain. The thermometer 50°. Very windy all day, with heavy showers. A loud clap of thunder about noon, which was heard, for several miles round this place; and in the neighbourhood of Warrington there were several flashes of lightning seen, accompanied with loud thunder. At three in the afternoon, the thermometer fell to 43° and to 40° at night. The clouds entirely disappeared in the evening, and the stars shone with a feeble lustre, indicating a great abundance of aqueous vapour in the air.

About half past eight a beautiful aurora borealis presented itself in the shape of a well-defined luminous arch, which crossed the northern heavens, and from which proceeded various groups of streamers; but nothing extraordinary was observed, though closely watched, till eleven o'clock. The arch, in this case, was nearly, if not exactly, at right angles to the true meridian.

Monday, February 19.—Stormy west wind, with heavy rain-clouds in the morning. Thermometer 47°; it rose to 50°, and much rain fell during the day. The wind continued high until evening, when it slackened a little, but still kept up a strong cold breeze. At night, there appeared an aurora borealis of the most extraordinary character hitherto recorded in the history of the meteor. It commenced with the close of the day, with a strong glare of light in the northern heavens, but without any definite shape or boundaries, and continued in this condition till nearly eight o'clock, about which time some faint colourless streamers appeared, and, occasionally, dim flashes of light swept across the sky, generally from east to west, and at a higher altitude than was reached by any of the streamers; but neither appeared to have any reference to the northern glare of light, which continued nearly steady from first to last. The horizontal span of this light reached from beneath the tail of the Great Bear, or about the shoulder of Bootes on the eastern side, to nearly the chest of Pegasus, on the western side; but the boundaries were so badly defined that no exact point in the heavens could be selected to mark the precise dimensions. The altitude of this northern light was quite as difficult to ascertain as its horizontal range, because of its gradual softening into the ordinary nocturnal colour of the sky. I can only say that it embraced α Lyræ and α Cygni, which were seen within it; the latter star just within its upper edge.

Such were the characteristics of the meteor till nearly nine o'clock, about which time commenced the first novelty in the history of the aurora borealis. A glow of light made its appearance close to the tail of the Great Bear, which waxed to a considerable degree of brightness, and after remaining for about half a minute, it gradually waned in splendour, until it finally disappeared. This spectacle had just ended, when a horizontal arrangement of short glowing beams, of the usual shape of streamers, began to parade the northern heavens, about half-way between the steady glare of light already described, and the Pole-star. They came into existence on the eastern side of the meridian, and marched very orderly, one after another, westward, in the same regular order of succession as they sprang into existence, until they reached a point directly beneath *Cassiopia's Chair*, where they became extinct, and were successively lost in the sky at the moment of their respective arrival at this spot, their apparent destination. This scene lasted several minutes, almost without interruption. During some part of the time the line of columns, between the two points in the heavens, was complete from one to the other, and had very much the appearance of an army of soldiers marching in single file, where the observer could just see them coming into view on his right, and vanish on his left; the whole marching past as if for his especial review. The bases of these luminous beams were flat and well defined, but the upper extremities were of a diffused radiant character, and gradually softened off till lost. During the time of this strange spectacle, several minor groups made momentary displays in different parts of the northern sky, and all seemed to move in the same direction, from east to west.

The next scene in the drama was partly similar to that just described, but of far greater splendour and extent. It began about half-past nine, at a point near the tip of the tail of the Great Bear, with a steady glow of pale light, from which issued an immense host of bright glowing beams, which marched across the meridian, with their centres at the altitude of the Pole-star, until they reached nearly to Venus. The movements of this grand array were slower than those of the first described columns, and also different in character. The first glided smoothly along without much vibratory motion; but these exhibited a kind of dancing or jog-trot sort of march; which appeared as regular as the march of an army of soldiers guided by a band of music; all hands, from front to rear, *keeping step* in a very orderly manner. The length of these beams much exceeded the length of the former group, and their upper extremities were so well defined that they formed, whilst the line was complete, a beautiful arch, the highest point of which was considerably higher that the Pole-star; but their lower parts shot downwards in variously-pointed terminations, like a series of *inverted* streamers, and were lost, at many different altitudes, amongst the stars, but never reached so low as the northern glow of light. Like the former group, these beams or columns sprang into existence individually, and in regular succession, from the same source, near the tail of the Great Bear, and took up the line of march from the commencement of their respective births, so that the individuals forming the moving line were every one of a different age, the foremost the eldest, and all the rest in the order of succession, from front to rear; and they appeared to vanish in the same order of succession, at a point in the heavens close upon the planet Venus, so that the last which sprang into existence in the east, kept its position in the rear of the line all the way to the west, and was the last that was seen, individually, in this part of the aërial spectacle. But this was not the conclusion of the scene; for instead of these luminous beams vanishing entirely, in the manner of the previous group, they seemed to assemble, in a close compact body, in the west, where they disappeared as individuals, and to form a broad luminous streak, which reached downwards almost to the horizon, and which, for a while, increased in splendour and dimensions in proportion to the number of beams assembled. This extraordinary streak of light continued in full splendour for about two minutes, when it began to waver; and its gradual decrease in both intensity and dimensions, until its final disappearance, formed the closing part of this second grand act of the meteoric drama.

We now come to the last, and by far the most magnificent spectacle of the whole. It began about ten o'clock. Its general character was similar to that last described, but its splendour and duration far exceeded it. The luminous beams, in this case, issued, as before, from a point in the heavens near to the tail of the Great Bear, at that time a considerable height above the north-eastern horizon, and formed an arched line of march (for a march it really was) above the Pole-star, reaching exactly to the Pleiades westward. The length of these auroral beams was greater than that of the last described group, and terminated, both upwards and downwards, in the manner that streamers usually terminate *upwards*. These aërial spectres seemed to form a division of grenadiers, when compared with the hosts that had preceded them, not only with respect to their magnitude, but also as regards the stateliness of their movement, which was truly solemn and majestic, and well calculated to furnish the sublimest imagery for the poet, and to store the imagination of the superstitious with the most awful portentions. From the well-known interpretations which the ancients have given to certain appearances of the aurora borealis, some persons have been led to think that the writer of the Second Book of Maccabees alluded to something of this kind:

"Through all the city, for the space of forty days, there were seen horsemen running in the air in cloths of gold, and armed with lances like a band of soldiers, and troops of horsemen in array, encountering and running against one another, with shaking of shields and multitudes of pikes, and drawing of swords, and casting of darts, and golden ornaments and harness."—Book ii., c. 5.

Although there appeared nothing like horsemen in the aurora I am describing, it was hardly possible to resist the idea of a very formal and well regulated march of soldiers, in single rank, being strikingly imitated in this very extraordinary display of the meteor, which concluded with a long broad streak of yellowish-white light, the upper extremity of which reached nearly to the Pleiades, and the lower almost to the horizon, forming a brilliant tail, as it were, to that group of stars; this association of the Seven Stars, and sloping streak of auroral light, was no inapt representation of the head and tail of a comet, only that the stellar group was too dull to represent the prominent part alluded to, being completely thrown into the shade by the refulgence of the auroral light.

This streak of light continued for some minutes in nearly the same position, and gradually faded away in the same part of the heavens as that in which it was formed. It began in the same manner as the streak of light previously described; that is, on the arrival of the first beams of the group, and gradually waxed in splendour in proportion to the number accumulated at this terminal of the line, until the arrival of the last beam; and after shining in full glory for a short time, it gradually waned, until finally lost amongst the stars. With this grand spectacle the most interesting part of the aurora terminated; but a glow of light illuminated a great portion of the northern heavens the remainder of the night, and until three or four o'clock next morning. The wind was strong from the west, and piercing cold during the whole night.

(*To be concluded in next Number.*)

On Oceanic Infusoria, Living and Fossil.

"The improvements effected of late years in the microscope," says Dr Harvey, in his interesting volume just published,* "may well be said to have opened to us a material world of whose existence we should otherwise be wholly ignorant. The number of species of animals and plants now known, whose forms are so minute that they are individually invisible to the naked eye, and only appreciable when collected together in masses, is very great; and the catalogue is daily enlarging as the waters of the sea, and of lakes and ponds, are more carefully subjected to examination. What to the naked eye seems like a green or brownish slimy scum, attached to the stalks of water-plants, or floating on the surface of stagnant pools, displays to the microscope a series of elegant and curious forms, endowed with a most perfect symmetry and delicate structure of parts, each acting in the circle of its narrow sphere as perfectly as the more bulky creations above it. The great work of Ehrenberg has made the forms of many of those curious creatures sufficiently known; and a most elaborate monograph of a portion of them,* recently published in this country, has added much to the general history of the subject, while it affords to British students exquisitely-accurate figures and careful descriptions of all the British species of the group illustrated. The plants included in this microscopic world are classed by botanists under two families, the Desmideæ, which exclusively inhabit fresh water, and the Diatomaceæ, a great number of which are marine. The forms of these minute organisms are strange; they exhibit mathematical figures, circles, triangles, and parallelograms, such as we find in no other plants, and their surface is often most elaborately sculptured. *Isthmia obliquata* is found in spring and early summer on the stems of many of the filiform algæ, where it forms little glittering tufts a line or two in height. It has been brought from many distant parts of the world, both of the Atlantic and the Pacific Oceans. Many other species accompany it in our own and other seas. The *Licmophora* or *Fanbearer* is one of the most beautiful of our native kinds, and is very common in April and May on the leaves of Zostera, as well as on many of the smaller algæ. It is very generally distributed round the British Coasts, forming gelatinous masses of a clear brown colour on the plants it frequents. Under the microscope, however, its colours are much more gay, a yellow shade, variously banded and marked with deeper-coloured spots, tinging the fan-like leaves, which are borne on slender threads transparent as glass. The pieces or joints of which these plants are composed are called frustules; and each frustule consists of a single cell, whose coat is composed of a very delicate membrane made of organised silex. That these plants have thus the power of withdrawing *silex* or flint earth in some manner from the waters of the sea, and fixing it in their tissues is certain, but the exact method in which this is effected has not been ascertained. A remarkable point in their history results from this power of feeding on flint. It is this: their bodies are indestructible. Thus their constantly accumulating remains are gradually deposited in strata, under the waters of the sea, as well as in lakes and ponds.

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* The Sea-Side Book; by W. H. Harvey, M.D., Member of the Royal Irish Academy. Van Voorst, London, 1849.

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* *Ralfs on British Desmideæ.* London, 1848. Thirty-five coloured plates.

At first the effect produced by things so small, thousands of which might be contained in a drop, and millions packed together in a cubic inch, may appear of trifling moment, when speaking of so grand an operation as the deposition of submarine strata. But as each moment has its value in the measurement of time, to whatever extent of ages the succession may be prolonged, so each of these atoms has a definite relation to space, and their constant production and deposition will at length result in mountains. The examination of the most ancient of the stratified rocks, and of all others in the ascending scale, and the investigation of deposits now in course of formation, teach us that, from the first dawn of animated nature, up to the present hour, this prolific family has never ceased its activity. England may boast that the sun never sets upon her empire, but here is an ocean realm whose subjects are literally more numerous than the sands of the sea. We cannot count them by millions simply, but by hundreds of thousands of millions. Indeed, it is futile to speak of numbers in relation to things so uncountable. Extensive rocky strata, chains of hills, beds of marl, almost every description of soil, whether superficial or raised from a great depth, contain the remains of these little plants in greater or less abundance. Some great tracts of country are literally built up of their skeletons. No country is destitute of such monuments, and in some they constitute the leading features in the structure of the soil. The world is a vast catacomb of *diatomaceæ ;* nor is the growth of those old dwellers on our earth diminished in its latter days.

These earliest inhabitants of the world seem destined to outlive beings of larger growth, whose race has a definite limit, both ends of its existence comprised far within the duration of a species of *diatomaceæ.* Many of the existing species are found in a fossil state, even in early beds. No part of our modern seas is without this ever-springing vegetation. Of this fact, the late antarctic expedition* afforded many striking proofs. One of the objects of that expedition was to obtain soundings of the deep sea ; and these were made at depths which would have engulphed Chimborazo in the abyss ; yet the lead constantly brought up *diatomaceæ*, even if nothing else. Nor did the eternal winter of the antarctic sea diminish the number of these vegetables. Other sea-plants ceased at Cockburn Island, in the low latitude of 64° S. · and, thenceforward, the *diatomaceæ* formed the whole vegetation. The icy wall, called Victoria Barrier, which, at length, stopped the southward progress of the intrepid navigators, was found embrowned with them. Floating masses of ice, when melted, yielded them in millions. In many places they formed a scum on the surface of the icy sea.

(*To be continued in our next.*)

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* See Hooker's "Flora Antarctica," vol. ii.

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On Grooved and Striated Rocks in the Middle Region of Scotland. By CHARLES MACLAREN, Esq., F.R.S.E., &c. (With a Map.) Communicated by the Author.*

Sir James Hall first called attention to the polished and striated rocks in the valley of the Forth, in a remarkable paper, read before this Society in 1812, and printed in the seventh volume of our Transactions. He specifies four localities where he observed them: At Torwood, about 4 miles north-west from Falkirk; at Corstorphine Hill; at Fenton Tower, in East Lothian; and at Fass Castle, in Berwickshire. With the striated rocks at these places he associated a peculiarity of form, which is conspicuous in nearly all the low hills of the same district, namely, that they present crags of bare rock on their west, and deposits of soil on their east sides. To this peculiarity he gave the descriptive name of "Crag-and-Tail." The direction of the striæ was nearly east and west at Corstorphine Hill, WNW. and ESE. at Torwood, Fenton Tower, and Fass Castle. He found examples of crag-and-tail also in the west and south of Scotland, but with different bearings. In the former, the crag faced the east; in the latter, it faced the north. After careful consideration of the facts, aided by collateral lights derived from other natural phenomena, he concluded that the striæ and grooves, and the smoothing of the surface, to which he gave the name of *dressing*, must have been produced by fragments of rock, gravel, and sand, driven over the land by a great wave, or succession of waves, rushing from the Atlantic in an east or south-east direction; that one part of the wave passed right across Scotland, grooving the rocks in its course, laying bare the western fronts of the hills over which it swept, and depositing tails of soil in the sheltered spaces behind them; that another portion of the wave was arrested by the high mountains, and turned back, flowing off to the sea, westward or southward, producing groovings on the rocks, and the phenomena of crag-and-tail, in directions corresponding to the course of the return waves.

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* Read before the Royal Society of Edinburgh on 2d April 1849.

This theory bears the stamp of the acute and original mind of its author, and it offered perhaps the best explanation of the phenomena, which the range of geological information at that time could supply. In the same year we find specimens of striated rocks and crag-and-tail noticed by Colonel Imrie, in his paper on the Campsie Hills, in the second volume of the Wernerian Society's Transactions, and an idea somewhat similar as to their origin thrown out. Sir James Hall's explanation of the phenomena was pretty generally accepted by geologists in this country; and it is still, I believe, adopted, though perhaps in a modified form, by some able men. I shall notice very briefly a few of the leading objections to it.

1*st*, Striæ must have been produced by a sliding motion, like that of a plane or graving tool, while stones propelled over a firm surface, by a current of water, would have a rolling motion, which might polish the rocks, but could not cut groves in them. 2*d*, Supposing stones impelled by water to cut grooves, these grooves would not occupy such positions as we find them in, on sloping surfaces like the steep sides of valleys; the force of gravity would render them more or less inclined, while, in such situations, we find them horizontal. 3*d*, The groovings so cut would be deflected to the right or left, by slight inequalities of surface, and would not possess that wonderful straightness and parallelism which they generally exhibit, and which Mr Lyell has seen extending over a length of 100 yards in the United States. 4*th*, A great wave or debacle of the magnitude assumed, coming from the west or north-west, would have filled up deep valleys transverse to its course, like that called the Great Glen. Now, that glen, so far from being filled up, has a depth of 770 feet in Loch Ness, measured from the surface of the water,—a depth exceeding that of the German Ocean. The fact, that this deep fissure has not been filled up, is presumptive evidence, that no such wave has ever passed over the island. *5th*, The cause assigned does not explain how boulders weighing many tons were carried from the Grampians across the central valley of Scotland, the bottom of which is only 200 feet above the sea, and deposited on the Pentlands, at spots 800 feet higher. A current of water, however powerful, would have dropt them in the low country. *6th*, The debacle does not explain other distinct traces of the action of water upon our hills. Mr Chambers, in his recent work, has shewn that satisfactory evidence exists of the presence of the ocean in its proper form of a horizontal sheet of water, up to 1500 feet above its present level. Had this fact been known, and carefully studied, Sir James Hall would have been spared the necessity of resorting to a great hypothetical Atlantic wave.

No agent yet known but ice, or ice conjunctly with water, seems capable of explaining the phenomena for which Sir James Hall called in the aid of a *debacle*. Those who have read the excellent works of Professor Forbes and Professor Agassiz, are aware that a glacier, during its slow progressive motion, transports vast masses of rock over a distance of many miles; secondly, that it grooves and polishes the bottom and sides of the valley containing it, by means of the stones and gravel which it brings down; and, thirdly, that many of these stones are themselves grooved by the attrition they have undergone in sliding over the fixed rocks. We know also, that as floating ice lifts large stones from the bottom and sides of rivers, or the shores of the sea, and carries them away, it may leave striæ on rocks over which it passes. Mr Lyell found well-marked striæ cut on a rock in the Bay of Fundy, which he attributed, on good grounds, to the packed ice of the preceding season, or of a period very little farther back. The pack ice accumulates there to the depth of fifteen feet.* If this was effected on the shore by so small a mass, it is easy to conceive that our plains might be grooved and abraded by icebergs, armed at bottom with stones or gravel, and floating in a sea 500 or 1000 feet deep. These moving mountains of ice are known to have reached to a greater depth than this.

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* Travels in North America, 1845, vol. ii., p. 173.

If the grooves, scratches, and polishing, seen on our rocks, were produced by ice, those in the deep valleys must be due to the action of glaciers, which are found in the Alps to glide downward at the rate of one or two feet per day, with gravel, stones, and sand adhering to their bottom. In this case the largest grooves should be on that side of prominent rocks which is toward the head of the valley, and this, it will be seen, holds true in Scotland. On the other hand, if the scratches, grooves, and polishing, were caused by an irruption of the ocean from the west, it is evident that the direct wave setting eastward would be vastly more powerful than the indirect or return wave, produced by the supposed recoil of the water from the hills, and setting westward. It follows, that in the west of Scotland, where the effect of both waves must be best seen, the grooving and abrasion should be greatest on the west side, and least on the east side, of exposed rocks. It will be found that the case is just the reverse.

Glaciers are rivers of ice, which have their source at a higher level in the mountains, in what the Swiss call *Mers de Glace*, or "Seas of Ice." To account for glaciers in the valleys shortly to be noticed, we must suppose that Scotland, at some former period, had a climate as cold as Labrador or Greenland, and that a permanent envelope of ice and snow covered all the higher region of the Grampians. There is nothing extravagant in the magnitude assigned to this envelope, for Agassiz informs us, that among the numerous mers de glace in the Alps, there are some 20 or 30 leagues* (50 or 75 miles) square. Glaciers or efflux streams of ice from this central mass would glide slowly downward through the openings at the outskirts of the mountains, such as the valleys of Loch Fine, Loch Long, Loch Etive, Loch Earn, and others; and if the hypothesis be correct, the sides and bottom of these valleys should be grooved and abraded, and the marks of abrasion and grooving should be most conspicuous on that side of prominent rocks facing the head of the valley. We shall see how far this conclusion is confirmed by facts.

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* Agassiz *Etudes Sur les Glaciers*, p. 22.

I begin with Gareloch, because the markings there are peculiarly distinct; and I was able to examine them more carefully than those of any other locality. It will be sufficient here to give a condensed outline of the two papers I published in 1845.

Gareloch.—The arrows 8 and 9, in the map (Plate II.), indicate the direction of the striæ and groovings here. They all point SSE., corresponding very correctly with the axis of the loch. They are very numerous, and while some of them are fine scratches only visible when the surface is wetted, others are grooves several inches, or even feet, in breadth. There is one face of rock 8 feet high and 2 feet broad, in a position not far from vertical, which is entirely covered with groovings, generally about an inch broad, and nearly horizontal. The markings are found several feet *under the high-water line*, and, though most abundant in low positions, some may be traced at a height of 300 feet above the sea, and three grooves were seen at an elevation of 600 feet, on the top of the ridge which divides Gareloch from Loch Long, and quite conformable in their bearing with those below. The grooves are cut on the edges of the laminæ of the mica-slate; and all those portions of the surface which are not striated are smoothed as if by abrasion. There are many dome-shaped prominent rocks; and the sides of these which face the head of the valley, are more abraded than those which look in the opposite direction, shewing that the abrading and grooving agent (*agent sulcateur* of the French geologists) moved from the NNW. to the SSE. Masses of rocks weighing many tons have also been moved in this direction. Stones with striated surfaces are found on the beach; and on the east side of the valley, at a height of 500 feet, fragments of what seems to be a lateral moraine are visible. In short, marks of the ancient existence of a glacier in the valley are numerous and remarkably complete.

Loch Long—Arrows 7 and 8.—I found grooves on t
beach, on the east side, at a hamlet called Letter, though t
coarse texture of the slate here is ill fitted to retain the
They run parallel to the line of the loch. A friend of mi
saw others on the west side, a mile or two north of H
Loch. But the most conspicuous and characteristic are o
vertical surface of rock on the west side, immediately below t
junction of Loch Goil with Loch Long They are horizont
from one to two inches broad, and cover some square yar
When the rock is wet they are seen from the deck of
Loch Goil steamer at the distance of 50 feet. Large groo
of this kind, on a vertical surface (and the examples are
rare), as they could only be produced by an immense *late*
pressure, acting at right angles to the force of gravity, se
to me of themselves conclusive against the hypothesis wh
ascribes their production to currents of water charged w
stones and gravel.

The local position of these well-marked grooves seems
illustrate an opinion lately put forth by Agassiz, Desor,
Charles Martins. They say it is necessary to the format
of a glacier that a cavity (*cirque*, *amphitheatre*) much wi
than itself should exist above it on the mountain, to serve
a reservoir for the collection of the ice and snow which f
it.[*] Now, Loch Goil meets Loch Long at an angle of ab
40°, forming with it a figure resembling the letter Y. Wh
ice and snow filled both valleys to the depth of 1200 f
the comparatively low hill in the bifurcation, called Argy
Bowling-Green, would be nearly covered, and the upper p
tion of the valley would form a reservoir or mer de glace
or seven miles wide, such as Agassiz describes. But tak
them in their present state, each of the lochs, before th
join, is as broad as the united loch after the junction; an
they were filled with ice moving slowly southwards, that
would be powerfully compressed when the united mass
forced into a channel only half as broad as the two chann

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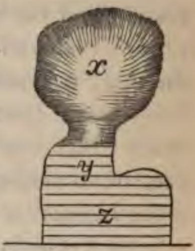
[*] Paper by Charles Martins, Edinburgh New Philosophical Journal,
lxxxv., p. 54.

it had previously occupied, and would exert an immense lateral pressure on the walls of the valley confining it. Hence it is in situations like this that deep groovings, and especially on vertical surfaces, should be looked for.

Something of the same kind is seen at Gareloch, which has a form resembling the figure annexed. Striæ and grooves abound in the circular valley *x*, where the ice must have collected; those in the bottom are few and large; those on the sides numerous, but generally small. At *y*, where the valley is contracted to a gorge half a mile in breadth, the bottom, being covered with salt water, can no longer be seen; but great numbers of striæ are found on the sides, and of various sizes, up to 6 or 8 inches in breadth. At *z*, where the loch widens out to a mile in breadth, and where the lateral pressure would, of course, be greatly relaxed, the striæ disappear, and are no more seen till we come to Row, five miles southward, where the breadth of the loch is again contracted by the point of land at Roseneath. At this place a few are visible (one 16 inches broad) scooped out across the laminæ of the clay-slate, which here succeeds to the mica and chlorite slate.



The cavity *x* would serve as a reservoir for the ice, or *mer de glace*, when the glacier occupying the valley was small; but the grooves found on the top of the ridge dividing Gareloch from Loch Long (on a surface wonderfully smoothed and levelled) point to glacial phenomena on a grander scale. They can only be accounted for, in my opinion, by assuming that one vast mass of ice filled Gareloch and Loch Long, covering the ridge which divides them, and that the whole moved simultaneously in a SSE. direction, constituting a glacier four miles broad, and probably 1000 feet in depth.*

The smooth sides, and even or gently-undulating outlines of the hills between Gareloch and Loch Lomond, which contrast so remarkably with the rough surface and serrated ridges of the mountains northward, led me, at first, to think that the protuberances and salient points of the former had been ground off by icebergs. I had then no data in my possession, authorising me to conclude that glaciers ever attained the depth of 2400 feet, necessary to cover the ridge on the west side of Loch Lomond; but the objection on this ground is now removed. The able French geologist named (M. Martins), has found traces of an ancient glacier on the Alps, 758 metres (2468 English feet) above the bottom of the valley which contained it. There is no difficulty now, therefore, in admitting, that a glacier might abrade the surfaces of the highest of these ridges.

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* See my paper in Edinburgh New Philosophical Journal, No. lxxxiii., p. 35.

Loch Eck—Arrow 6.—The rocks on the two sides of this loch are smoothed and rounded off in a manner so conspicuous, that it cannot fail to strike the most careless observer. In a hasty journey through it, I saw no striæ; but the coarse surface of the slate is ill fitted to shew them. Dome-shaped rocks, however, with one side rough, clearly shew that the abrading agent moved in the same direction as at Gareloch,—namely, SSE.

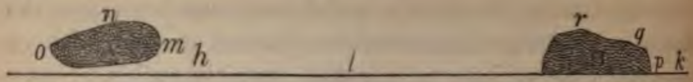
Loch Fine—Arrow 5.—I found some distinct groovings on the beach at St Catherines, opposite Inverary. Their bearing was conformable to that of the loch, or about SSW. It is worth remarking that there is a bifurcation here, caused by the meeting of two valleys, in the form of Y, like that which occurs at the junction of Loch Long and Loch Goil.

Loch Awe—Arrow 4.—Smoothed rocks of gneiss are numerous at the foot of the hill of Stobacherachrun, on the north side of the loch, and many of the islets in it seem to be low, abraded domes. On the south side of the loch, about a mile west from Dalmally Inn, there are two little hills on the right and left sides of the road, which exhibit the crag-and-tail form, on a small scale, in a position the reverse of that we are accustomed to. The east side of each is laid bare and smoothed, while a mass of stones and soil covers the west side. On the face of the hill lying on the left or south side of the road, I found a few grooves, which pointed ENE. and WSW. Taken in connection with the crag-and-tail, they indicate that the abrasion and grooving were produced by agents coming from WSW., where the deep glen, the Orchay, is situate, which may once have been the seat of a glacier. I found traces of broad grooves also on two small hills which rise abruptly from the valley, about a mile south from the inn.

Loch Etive—Arrow 3.—I examined only a small portion of the southern shore of this loch, extending about a mile and a half westward from Connel Ferry. The coast here is formed of basaltic clinkstone, which pushes out a series of salient points, sloping gently to the sea, and each resembling a segment of a *discus*. They are finely rounded and polished, though the rock is divided into thousands of polygons a few inches broad, by fissures, in which fuci have their roots. I found striæ on several of these points, in the space between high and low water, where the smaller fuci grow. They were narrow, about the breadth of straws, but were rendered quite distinct by washing the surface. They were all on that face of each *discus* which sloped eastward or south-eastward, and which was generally more abraded than the face which sloped north-westward. Their bearing surprised me. It was not conformable to the line of the shore—that is, east and west, but ESE. and WNW., as if produced by agents coming from Loch Awe, which is ten miles distant, and divided from Loch Etive here by hills from 300 to 500 feet high. Professor Forbes has shewn, that glacier-ice has a considerable degree of plasticity; and Agassiz infers, from the occasionally-oblique position of the striæ, that it mounts over obstructing masses of rock. Shall we, then, assume, that the basin of Loch Awe was filled with it to the height of 1000 feet, and that a portion of it might find its way over the broad hilly barrier in this direction? The facts yet observed are inadequate to support such a conclusion; but I give them as they presented themselves.

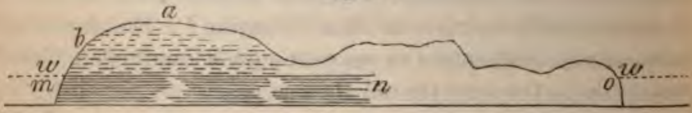
Loch Leven—Arrow 2.—About a mile westward of Balahulish Ferry are two small outliers of the granite mountain, which skirts the shore there. They are about 80 feet long, and rise about 6 feet above the high water level. The eastern has the sea on three sides, the western is an island at high water.

Fig. 1.



h l k is the line of the shore, which runs east and west; A the western rock, B the eastern. A portion of the surface of both rocks is grooved, and the position of the grooved surfaces is highly instructive. The north-eastern face of B, from *q* to *r*, which is nearly vertical in its lower part, and rounded off above, is almost entirely covered with grooves, which are horizontal, straight, and parallel, generally about one inch broad, and so uniform and close together, as to remind one of the flutings of a Doric column. They cease at *r*, precisely at the point where the rock begins to face the north-west; and the north-west, as well as the west face, is entirely ungrooved, but considerably abraded. The east face *p* is partially grooved. The islet A presents appearances precisely similar. The north face, and a portion of the east end,—that is, from *m* to *n*,—are beautifully marked with horizontal grooves, which entirely disappear on the part from *n* to *o*. The grooving agent, then, had power to cut furrows in rocks facing the ENE. and N., but no power to furrow rocks facing the WNW., or even NNW.,—so rigid and steady was its westerly motion. Could water act in this way? All the ungrooved sides of the rock are, less or more, smoothed, and the roughest part is the west end *o*. There is one distinct groove on the top of the islet 3 or 4 feet long, and 1½ inch broad, pointing exactly east and west.

Fig. 2.



The above is a view of the north face of the islet, drawn shortly after the visit, from memory, and not pretending to literal accuracy; *m n* the grooved part, *n o* the ungrooved part. It is a curious fact, that *the most distinct grooves are in the space between high and low water*. Above the high water line *w w* they become fainter and fainter, till they disappear. This holds true of both A and B. It seems strange that the action of the tide, which might be expected to obliterate the grooves, should be the means of preserving them. Such, however, is the fact; and perhaps it admits of explanation. The agent chiefly concerned in wasting the surface of the granite above the water, appears to be a sort of grey fog (a cryptogram, I suppose), everywhere visible, which takes root upon it, and, falling off, and being renewed periodically, carries minute grains of the rock with it, and thus gradually wears down the surface, and obliterates all fine markings upon it. The part beneath the high water line is protected from this vegetation by the tide, and still more, by a sort of black pigment which the sea deposits upon the rock. To the eye, it has exactly the aspect of a coat of paint, and is probably about the 100th part of an inch thick. When examined with a lens, it is seen to be divided by innumerable cracks into polygonal facets, from a 40th to a 200th part of an inch in breadth. This coating is probably permanent, or at least very durable; and it covers most of the large groovings which are under the high water level, apparently without rendering them less conspicuous. I found it also on the basaltic clinkstone at Loch Etive (arrow 3); but there it seemed to be confined to the parts of the rock near the high water line, and probably to those daily wetted by the spray. It was thick enough to conceal the fine striæ, which were visible at a greater depth.

These two granitic masses seem to me to present a crucial test for Sir James Hall's theory, and entirely to disprove his fundamental proposition. From the facts detailed, two conclusions inevitably result: First, that the agent which produced the grooves moved from east to west; secondly, that no agent capable of producing groovings, and acting contemporaneously, moved in the opposite direction, or from west to east. We have here the advantage so seldom obtainable, of proving a negative. Sir James Hall, according to his theory, would have said that the grooves on the east ends of A and B were produced by the recoil wave, thrown back by the mountains; but if the recoil or secondary wave, forming but a fraction of the great *debacle*, had sufficient power to do this, the direct and *primary* wave which immediately preceded it, consisting of the whole mass of water, and moving here freely over an open bay 5 miles wide, would have had much more. The west ends of A and B, then, should have been much more deeply grooved than the east; while, in point of fact, the former are not grooved at all. It is plain, therefore, that the hypothetical wave had no existence.

On a flat surface of the rock, at *l* in front of the granite quarry, striæ and grooves running E. and W., and beautifully distinct, cover several square yards. The north front of the granite hill presents abundant marks of abrasion, but I saw no grooves, though, doubtless, they once existed there.

On the edges of the clay-slate, which is quarried for roofing, 2 miles east of the ferry, there are large conspicuous grooves running horizontally at an elevation of probably 40 feet above the sea. Some of them seemed to be 5 or 6 inches broad.

Glen Spean—Arrow 1.—From the Catholic chapel, a short distance eastward of Glen Roy, to the granite hill towards Loch Laggan (represented by an oval, shaded space, on the map) a line of 4 miles, abraded rocks are very numerous: striæ were not common, but they were found at four or five places. They were horizontal, and on vertical or inclined surfaces. That the motion of the body which produced them was from east to west, might be inferred from the abrasion being greatest on the eastern sides of the rocks; but a more direct proof was afforded by the distribution of the granite boulders. These are scattered in thousands over the surface of the mica-slate for a mile westward of the granite hill, and of all sizes, up to blocks weighing ten tons. Smaller masses are found as far west as the bridge of Roy, and beyond it. Granite blocks are also met with on the terraces of the parallel roads. I counted twelve on two miles of the lower and second terraces, varying in bulk from half a cubic yard to two cubic yards. As they had lost their angles by weathering, specimens were not easily procured, but I was able to satisfy myself that some of them were identical with the rock eastward, alluded to. Others may belong to the granite mass seven miles northward, at the head of the River Roy. Mr Darwin found granite blocks on the hills between

Glen Roy and Loch Lochy, at the height of 2200 feet above the sea.

Loch Earn—Two arrows 14.—At the west end of the village of Comrie there is a broad platform or shelf of clay-slate, projecting ten yards from the hill above, and of which Fig. 3 is a section lengthways. It rises 25 feet above the road, is about 200 feet long from *a* to *d*, flat on the top *b c*, truncated at the east end, *c d*, but terminating in a beautifully rounded and smoothed declivity *a b*, at the west end. This form is shewn in the section, and prevails among the adjacent rocks. The platform, *b c*, exhibits a fine specimen of grooving. The whole area is smoothed, and the grooves appear at intervals over all the flat part of it, of which they cover a considerable proportion. They are from ¼ inch to a full inch in breadth, straight as mathematical lines, and everywhere rigorously parallel. Their bearing is WNW. and ESE., or more correctly N. 60 W., and S. 60 E., and they cross the planes of the slate at an angle of 25° or 30°. One space, 10 feet by 3, is entirely grooved.

Fig. 3.

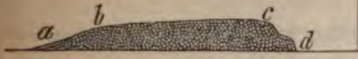
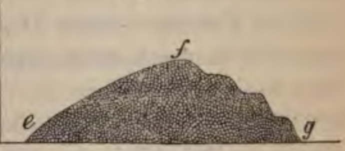


Fig. 4.



In the picturesque and beautiful district from Comrie to St Fillans, at the east end of Loch Earn, a succession of rocks occur on both sides of the road, which present smoothed and abraded faces, *e f* (Fig. 4), to the west, while the eastern side, *f g*, is rugged or uneven.

At the east end of the loch, on the south side, there is a section of the rock exposed, close to the cart-road. It is 10 feet long and 6 feet high, and is entirely covered with grooves from ¼ to 1½ inch in breadth, and nearly horizontal. The grooved area faces the NNW., crossing the axis of the loch at 15° or 20°, and is inclined to the horizon at 35°. It is about 5 or 6 yards above the level of the water. I found striæ also on the north side of the loch at two places, running east and west. The surface was wet with rain; in a dry day they would have escaped observation.

Loch Lubnig—Arrow 12.—Striæ were seen on the east side of the loch, close to the road, one mile from the south end. They ran north and south, were on a surface highly inclined, and would not have been visible if the rock had not been wet.

Loch Katrine—Arrow 11.—At one spot on the north side, near the east end, striæ were seen, running nearly E. and W. on a horizontal surface.

Callender—Arrow 13.—On the top of the hill, which rises like a wall behind the village, I found grooves running nearly east and west. The hill consists of beds of coarse conglomerate, mixed with beds of red sandstone, all very highly inclined. The grooves were on a portion of the edges of the sandstone, which was nearly level.

I consider the proofs of an easterly motion in the grooving agent at Loch Earn and Comrie, to be quite conclusive, and on the strength of this evidence, have assumed that the motion was easterly also at Loch Katrine and Callender, and southward at Loch Lubnig, as the arrows 11, 12, 13, indicate.

The dotted line in the map, extending from Bute to Crieff, and onward to the River Tay, shews the junction of the old red sandstone and clay-slate, and marks the eastern boundary of the mountainous country.

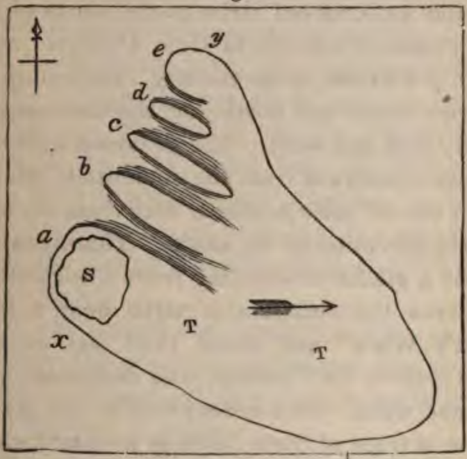
Horizontal groovings are seen at the west end of the Crinan Canal, on a vertical surface (No. 10), a little above the level of the water; but there is nothing to indicate in what direction the object which caused them moved.

In the basin of the Forth the striæ run in lines approaching to east and west; and the appearance of the hills, which present the phenomena of crag-and-tail, entitles us to conclude that the agents which produced the striæ moved from the west to the east. In the map an attempt has not been made, except in a few instances, to give the direction of the lines within less than one point of the exact bearing, many of the observations having been made some years ago, when a minute attention to this matter was not thought necessary. Some of them, too, are on vertical surfaces, and so placed as ndicate that the grooving agent was deflected from its ginal course. In the district generally, the uniformity of ection being so great, a mere list of the localities is nearly that is requisite.

rrow No. 15. On the west shoulder of Demyat, three es from Stirling, 500 feet above the sea, direction ESE.

6. At Torwood, four miles NW. from Falkirk, arrow 16, ection ESE., observed by Sir J. Hall. In this and the ceding, the bearing of the striæ corresponds with that of upper part of the Frith of Forth, and with the remark- e furrows on the rock of Stirling Castle, of which the re below is a rough sketch, borrowed from the "Sketch he Geology of Fife and the Lothians."

Fig. 5.



his rock forms an isolated hill, rising 300 feet, at S, ve the plain which surrounds it. The highest part is an arpment of trap, *a*, *b*, *c*, *d*, *e*, fronting the north-west. The row, or rather ravine, dividing the ridge *a*, from the ridge s about 60 feet deep, and sharply cut. The others, be- een *b* and *c*, *c* and *d*, *d* and *e*, are from 15 to 40 feet, and y all point north-westward. The coincidence in bearing hese furrows with the striæ on Demyat, 3 miles northward, with the others at Torwood (arrow 16) is interesting.

7. On trap, one mile south from Borrowstounness, about or 200 feet above the sea.

18. On sandstone, at Hillhouse Quarry, one mile south from Linlithgow.

19. On the shore at Granton Pier, nearly one point north of east—(*Dr Fleming*).

20. On Corstorphine Hill, nearly one point north of east—(*Sir J. Hall*); also at Ravelston and Craigleith Quarry, seen by myself.

21. On the north limb of Arthur Seat, 500 feet above the sea—(*Dr Fleming*); and on the Queen's Drive, south side of the hill.

23. Westward of Craigmillar Castle, exposed in a quarry some years ago.

24, 25, 26. On Pentland Hills. Few groovings have been found on the Pentland Hills, but those known are interesting. I found well-marked striæ on the banks of Westwater (arrow 26), about a mile north from Dunsyre, at an elevation of 800 or 900 feet above the sea. The valley, which was not deep, runs south and north, and the striæ crossed it, running exactly east and west. The grooving agent, therefore, did not move downward from the summits of the Pentlands, but crossed one of their southern declivities at an angle of 45°, with the direction of the chain. That agent, therefore, could not be a glacier descending from the Pentlands. Arrow 25 marks the situation of striæ near a place called "Thomson's Wa's," and about 1400 feet above the sea. They were seen by Dr Fleming, who described them as running east and west. On a recent visit to the place, I could not discover a trace of them; owing, no doubt, to the blocks of sandstone on which they were, having been removed in quarrying. They were on or near the ridge which constitutes the watershed, and about half a mile east from East Cairn Hill, whose height is stated to be 1800 feet.

Arrow 26. Very distinct striæ have been recently exposed about half a mile west from Bonally, where a reservoir is now constructing. Mr Leslie, the engineer who planned the works, obligingly called my attention to them. They occur on the north face of Torduff Hill, about 30 or 35 feet above the bottom (the *real* bottom of rock) of the deep and narrow valley between that hill and Warklaw Hill. The face of the rock (a felspathic claystone), dips at 40°; the striæ are horizontal, parallel, and quite straight, and extend over a surface from 1 to 3 feet in breadth (vertically), and about 25 feet in length. Their direction corresponds with that of the valley at the place, being precisely ENE. and WSW. The valley is about 300 feet in depth, and, including the upper portion, which curves round the west end of Torduff Hill, about three quarters of a mile in length. It is such a valley as might give birth to a glacier at a glacial epoch. On the top of the same hill, about 900 feet above the sea, striæ and grooves, in short lines, can be detected at intervals, pointing also very uniformly ENE. and WSW. A floating body, such as ice, coming hither from the west, would have a course perfectly unobstructed for 20 miles; for the high ground in a WSW. direction presents the aspect of a plain. Such a body, as it passed along, would graze the western front, the top, and the flanks of this hill; and accordingly we find that, like the hills in the low country, it has the crag-and-tail form, with the crag to the west. Both the head and foot of the hill exhibit proofs of abrasion and grooving, but whether by glacier ice, or floating ice, or both, is still a problem.

27. At Fenton Tower, direction ESE. and WSW., noticed by Sir James Hall.

28, 29. At Old Markle and Gosford Spittle, on the North British Railway, the groovings horizontal, and very distinct, but the surfaces are vertical, they seem to me to give no sure indication of the line of motion.

My principal object in this paper was to register the phenomena observed; and, in speaking of their probable causes, I shall endeavour to be brief.

The Grampian District.—We have seen that, on the east side of this district, at Loch Tay, the abrading and grooving agents moved eastward; that on the west side, at Glen Spean, Loch Leven, and Loch Etive, they moved westward; and that on the south side, at Loch Fine, Loch Eck, Loch Long, and Gareloch, they moved southward. It follows that the nucleus of this physical force, the common centre from which the agents moved, was in the group of mountains extending from Loch *Goil north*ward to Loch Laggan, dividing the springs of the Spean, the Leven, and the Orchay, from those of the Spey, the Tay, the Earn, and the Forth. And the force must have resided in some substance which admitted of accumulation to a vast extent, for the abrasion produced by it can be traced to the height of more than 2000 feet. Now water could not be so accumulated here *except in the form of snow and ice*, and even if it were so accumulated in the liquid state, it could not, as has been shewn, produce the effects fairly ascribable to it. These effects are such as cannot be ascribed to any agency known, except that of glaciers, aided perhaps, in some cases, by floating ice.

Professor Forbes, in his paper on the Cuchullin Hills in Skye (in No. 79 of this Journal), describes well-marked groovings on their sides, radiating from the hills in different directions, as from a common centre, so disposed, he observes, that they can be accounted for neither by mountain lakes nor great oceanic waves, nor by any great agent known but glaciers.

I have not yet examined the channels of the Spey, the Findhorn, or the Dee, but I have no doubt that groovings pointing northward and eastward, will be found in them. I infer that the mountainous country, west of the Great Glen, from Morvern northward to Sunderland, was another centre of glacial action; and further, that the Great Glen itself was probably the seat of a glacier which found an exit by its north and south ends, and was fed by the smaller glaciers flowing into it from the east and west.

The striæ, groovings, and kindred phenomena, in the great central valley between the Frith of Forth and the Frith of Clyde, and on the hills contained in it, do not seem to admit of explanation on precisely the same principles. The striæ in this district have a direction always approximating to east and west, and there is good evidence to shew that the abrading agent moved eastward. No glacial markings have yet been discovered, so far as I know, running in lines at right angles to the sides of the Pentlands, such as glaciers in the transverse valleys would produce. On the other hand, the striæ found on their summits and flanks (arrows 24, 25, 26), either run along the chain, or hold their course independently of it.

Much remains yet to be done before adequate materials for a satisfactory theory are collected. In the mean time, a few conjectures may be indulged in provisionally.

The transportation of a block of mica slate, weighing 8 or 10 tons, from the Grampians, across the low land, to a point in the Pentlands 1000 feet above the sea, is scarcely susceptible of explanation, except by calling in the agency of ice floating on an ocean at a far higher level than the present. The existence of such an ocean, with masses of ice floating on it, whether in the shape of icebergs, field-ice, or coast-ice, being admitted, it seems a legitimate inference, that the ice, borne eastward by a current, and having probably stones and gravel adhering to it, or imbedded in it, might produce the striæ on the top of Torduff Hill, arrow 24, and those at the other high localities 25 and 26. Farther, as the ocean, in ascending to its higher position, or descending from it, must have assumed different levels in succession, the striæ on Arthur Seat, and Corstorphine and Ravelstone Hills, and at all the other localities, high and low, from Stirling to Gosford and Fenton Tower, might be the result of the same agency. This seems a more reasonable hypothesis than that which assumes, that a vast sheet of ice covered the country from the Grampians to the Lammermuirs (a breadth of 50 miles), and, in moving eastward, grooved both the high lands and the low. It seems to afford a better explanation of the phenomena.

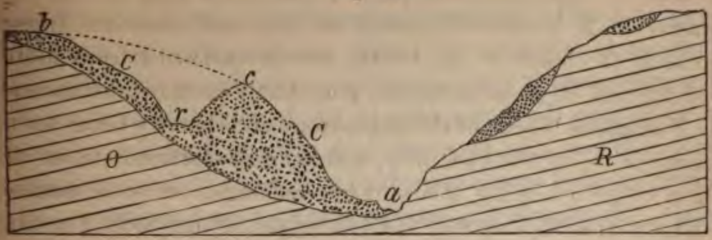
The craig-and-tail form is so often accompanied with groovings, that it is due probably, in a greater degree, to floating masses of ice than to the current which bore them along.

There is a class of phenomena best accounted for by the agency of coast-ice, which is well known to lift stones and gravel from the bottom and sides of rivers and bays, and transport them over moderate distances. Mr Lyell cites examples of blocks weighing 50 tons, being removed in this manner by the ice of the St Lawrence. In this way we may explain such facts as the following. 1. Thousands of granite blocks lifted from the hill in Glen Spean (arrow 1), near Loch Laggan, and carried westward; a vast number of them dropped within a furlong or half a mile of their original site, a smaller number conveyed a mile, and a few to much greater distances; here, however, part of the effect may be due to glaciers. 2. Travelled masses of trap and other enduring rocks in the basin of the Forth, carried eastward from their parent rock in great numbers, and chiefly for short distances. 3. Numerous blocks of the greenstone of Salisbury Crag, torn from the top of the precipice, and carried eastward, most of them only for a space of one or two furlongs, but some transported across the ravine, and lodged on Arthur Seat. On the same principle, the removal of the multitude of angular blocks of porphyritic basalt, resting on the skirts of the south-east limb of Arthur Seat, and evidently torn from the upper part of that limb, may be accounted for.

Rarity of ancient moraines.—I looked in the grooved valleys of the Grampians for remnants of ancient lateral moraines, but saw nothing that could be considered as such, except in one instance at Gareloch. Perhaps their disappearance may be referred to certain geological changes, of which, in the opinion of Agassiz, and some American geologists, distinct traces exist. They think that at the close of the glacial epoch the sea rose and covered the mountains of the northern parts of Europe and America to a great height, and then again subsided and left the land dry as before, though not perhaps at the same level. During the rise and fall of the water, deposits of moveable matter, like these moraines, must have been very often remodelled or swept away. We have evidence in support of the alleged changes of relative level in the fact that striæ and grooving, certainly produced by glaciers on *terra firma*, are found covered by the old boulder clay, which has been deposited from water, and which ascends to the height of 800 feet at least above the present seas.

A similar inference may be drawn from facts which the beds of our rivers present, and which indicate three successive conditions. First, the bed was a channel cut on the dry land by the stream; next, the land was submerged, and the channel was filled up by the boulder clay; thirdly, the land rose again above the sea, when the river began to resume possession of its old channel, or in some instances, perhaps, formed a new one. I refer, as an example of these changes, to a section on the River Allan near Stirling.

Fig 5.



The rock O R consists of beds of conglomerate and sandstone dipping to the west at an angle of 5° or 10°. The ravine at the Bridge of Allan is from 150 to 200 feet deep. C C is a deposit of clay, of which the lower half has distinctly the character of the older diluvium, being very firm, and inclosing striated blocks of chlorite slate and other travelled stones. It descends to the water-course at *a*, and the deep cut *b r c* made on it for the railway, which occupies the hollow *r*, assures us that the clay is not a mere superficial covering which may have slid down from above and concealed the face of the rock, but the remnant of a deposit which once filled, or nearly filled, the ravine. The depth, in the direction *r c*, is, at least, 80 feet. The rock is not visible on this side, but it reappears in a quarry half a mile westward with the usual dip, at an elevation exceeding that of the point *c*, and is also seen on the left of the railway farther north. The legitimate conclusions deducible from the facts, I think, are these; that the ravine was excavated in the rock by a river, and nearly to its present depth; that the land then sunk under the sea, and remained there during the deposition of the older and newer boulder clay, which filled up the ravine wholly or partially; that after this the land rose again above the water, when the river sought out and re-opened its old channel.

Examples of similar phenomena are probably not rare. There is a mass of dark coloured clay, 40 feet in height, forming the south bank of the Water of Leith at Coltbridge, which seems to indicate that that portion of the bed of the stream was excavated before the diluvium was deposited. It is alluded to in Sir James Hall's paper. In the parish of Muiravonside, westward from Linlithgow, the River Avon flows between two precipices of the old boulder clay from 100 to 150 feet in height. For the space of a mile above Side-trees, no rock is visible in the water-course, except a vein of trap at a mill; the distance from bank to bank varies from a furlong to a quarter of a mile, and the clay is so hard and tenacious that it rises at several points almost vertically, like a wall, from the water edge to the height of more than 100 feet. Where the clay was laid open by lateral rivulets, I found grooved stones embedded in it, some of them of chlorite slate. Farther south, sandstone is seen on both sides. The ravine seems, in short, to be an ancient water-course re-opened. The absence of visible rock on the west side, behind the clay, may, indeed, suggest the idea that there is nothing but clay in that direction; but the height and form of the land there, and other circumstances, satisfy me that this is not the case. Perhaps some of the ancient channels, when filled up with the boulder clay, were in the condition described by Playfair, namely, chains of lakes connected by streams whose channels abounded in cataracts. If the idea here thrown out be correct, that some of our rivers are now flowing in ancient channels reopened, it follows that the subsidence and re-elevation of the land through a space of more than 1000 feet, had done very little to disturb the levels. But the subject yet requires investigation.

On a Simple Form of Rain-Gauge. By the Rev. JOHN FLEMING, D.D., &c., Professor of Natural Science, New College, Edinburgh. Communicated by the Author.*

The defects with which rain-gauges may be charged, at present, seem referable to inattention to the influence of the wind on the falling *rain-drop*. If the drop was influenced only by gravity in its descent to the earth, the form and position of the rain-gauge would be comparatively of little importance. But in addition to its centripetal tendency, regulated in velocity by its size and the height of the fall, the rain-drop is frequently acted upon by the wind, and deflected more or less from its normal path, according to the velocity and direction of the current. While the wind thus influences the rain-drop, it likewise, in its turn, is modified in its horizontal direction by every projecting obstacle, and deflected, according to circumstances, laterally, upwards, or downwards, carrying the rain-drop along with it in its course. Whoever has watched the falling of rain under the influence of wind, and in the neighbourhood of houses, walls, or other obstacles, must have observed, as the result of the eddies generated, that it is deposited in defect in some places, and in excess in others. In the case of falling snow, the derangement is of the same character, but more obvious. Had these influences been duly attended to, there would have been fewer confident assertions respecting the smaller quantity of rain which *falls* on elevated buildings than at lower levels, and more inquiry respecting the cause of a less quantity being *collected* in such circumstances.

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* Read before the Royal Society of Edinburgh, 16th April 1849.

When a rain-gauge is elevated three or four feet above the level of the ground, it is easily observed and emptied of its contents, and, in calm weather, may be considered trustworthy. During a moderately stiff breeze, however, the rain-drops may be seen whirled about in the funnel, and even carried out and lost after they had nearly reached their destination. But independent of the eddies in the funnel, there are deflections of the current produced externally, which exercise corresponding influence.

The late Mr Thom of Ascog, Bute, a well-known and judicious hydraulic engineer, was in the habit of measuring the fall of rain, in order to predicate respecting the quantity of water which might be derived from a natural or artificial lake or pond, as a motive power for mill purposes. The gauge which he employed was similar to the one figured by Cavallo, in his "Natural Philosophy," vol. ii., p. 424, tab. xv., p. 3. It was defective, however, in the want of a rim to the funnel, so as to prevent the dispersion of any drops impinging directly on the sloping sides. But if defective in respect of the rim, the *position* which he assigned to the gauge itself, namely, placing it in a grass plot, and on a level with the surface, constituted a decided improvement. The mouth of the funnel, although unnecessarily large, does not present space enough to permit any perceptible acceleration of the current of wind, on the free surface, which had been uniformly retarded on the grass plot, and consequently receives a fair proportion of the falling rain.

The body of the gauge, for receiving the water collected by the mouth or funnel, by being placed in the ground, is protected against changes of temperature; little or no evaporation can take place, so that the emptying and adjusting may be effected at distant intervals.

Having employed for several years, at Aberdeen, an instrument presented to me by Mr Thom, I was satisfied that it fulfilled nearly all the conditions of a trustworthy instrument, differing, however, in its humble appearance, from those eye-traps or gimcracks usually set up as *rain-gauges.* The process of emptying, however, was a troublesome one, as the funnel required to be removed, the float lifted out, and then the water in the cylinder taken up by means of a cup or sponge, at the end of every month or two, according to circumstances.

To remedy this evil, it occurred to me, that by employing an external cylinder, permanently placed in the ground, for receiving the cylinder forming the rain-gauge, a considerable improvement might be effected. 1. The necessity of removing the funnel at every adjustment, might be got rid of by having a stopcock at the bottom of the receiver, so that upon the gauge being lifted out of the ground, or rather out of its external case, the water might, without trouble, be let off, and the float adjusted to zero, by the addition of the requisite quantity of water by the mouth of the instrument. 2. By getting quit of the funnel, the whole gauge may be a single cylinder, with the mouth of the same diameter as the body of the instrument, whereby errors of workmanship, producing unequal areas, may be avoided. 3. By simplifying the instrument, and thereby greatly reducing the expense, it is expected that observers will be increased, and additional data procured for determining the distribution of rain over the globe, by details furnished by comparable gauges.

The preceding remarks will render any minute description of the instrument unnecessary, but the following notices may be of use.

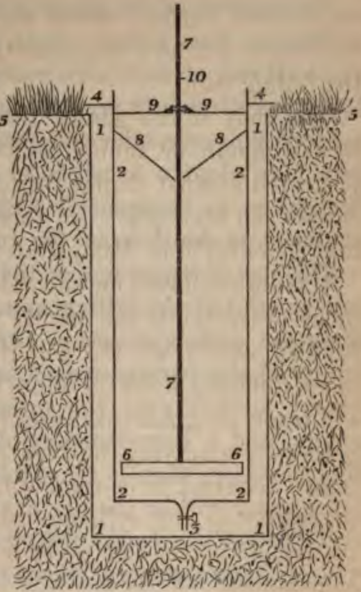

Fig. 1 is the external cylinder, closed at bottom, and sunk perpendicularly into the ground, with its margin on a level with the earthy surface. Fig. 2 is the inner cylinder or rain-gauge, open at top, to act as a funnel for receiving the rain, but closed at the bottom, except the central stop-cock, fig. 3, for letting off the water. This inner cylinder or gauge, is no narrower than its case, unless in so far as to allow of its being easily lifted out for the purposes of adjustment. This inner cylinder is furnished with a shoulder or flange, fig. 4, to close upon the mouth of the *case*, and prevent the entrance of any earth, sand, or leaves, so as to obstruct the easy elevation of the gauge.

Fig. 5 is the surface of the grass plot in which the gauge is placed, and which must be kept in a trim condition in its immediate neighbourhood.

Fig. 6 is the hollow float to which the index-rod 7 is attached. The rain which falls into the mouth of the cylinder will be conducted by the funnel, fig. 8, situate an inch and a half within the margin (to which it is soldered) into the receiver, fig. 2, and raise the float to a corresponding degree.

Fig. 9 is a thin vertical strip extending across the middle of the mouth, with a sheath at the centre to embrace the rod, as a *guide*, and to serve as determining the zero of the scale and the indications of change. When the stopcock, fig. 3, has been open, and the water let off, the beginning of the scale on fig. 10 will usually be a little lower than the marking edge, fig. 9. In this case, a little water is poured into the cylinder, in order to bring the commencement of the scale to zero, or fig. 10, and, on the falling of rain, the index rises, and, being divided into inches and tenths, numbered downwards on the rod, the quantity is readily seen by inspection.

The *position* of the gauge in a well-trimmed grass plot, at a distance from houses, walls, or trees, seems to me to obviate all risk of any extra water getting in, as the points of the grass effectually prevent any such occurrence. Those observers, however, who are particularly fastidious on the subject, may adopt a brush to be placed around the margin of the gauge, as recommended by Mr Thomas Stevenson, civil engineer, in this Journal for July 1842.

The *size* of the gauge, or the area requisite for receiving a fair amount of the falling rain, can scarcely be said to have attracted sufficient notice. If we assume that the acceleration of the current would take place, by passing from a grassy surface over the open space of the mouth of the gauge, and a corresponding derangement of the motion of the rain-drop, then it must follow, that the larger the area, the greater will be the amount of error from this source. In order to put this to the test, I placed in the same grass-plot with a Thom's gauge of seven inches in diameter, three other gauges of one, two, and three inches in diameter, and obtained the results for three months, viz.:—

		1842.	
	Sept.	Oct.	Nov.
No. 1. Diam. = 1 inch,	3·689	3·086	5·3255
2. ... = 2 ...	3·551	3·261	5·2589
3. ... = 3 ...	3·921	3·346	5·1829
4. ... = 7 ...	3·750	3·300	5·3750

The grass-plot was not free from eddies, arising from buildings, trees, and walls; but as all the gauges were nearly similarly exposed, and as they gave nearly similar results, I am inclined to think, that a receiver of one inch in diameter is as trustworthy as one of seven inches. When the instrument is to be made of copper (the most suitable ingredient), the small size reduces the expense. I have noticed the index-rod as divided into inches and tenths. The eye, after a short practice, has little difficulty in halving or quartering the tenths; and this is a degree of accuracy as great as the circumstances of the case warrant us in aiming at. The inequality in the fall of rain, at two places within less than a mile of each other, forbid us to expect any very accurate correspondence among gauges, even at moderate distances apart, although similar in form and position. The gauge exhibited to the Society, and which was three inches in diameter, and two feet in depth, was constructed for me by Mr James Bryson, Princes Street, Edinburgh, who has furnished several similar instruments, now in operation in different parts of the country.

In conclusion, I may add, that the average annual fall of rain at Aberdeen, according to six years' observations with Mr Thom's gauge, was =30·4 inches. According to Mr Thom, the average of thirty years at Rothesay, in Bute, was =48·29 inches. The maximum annual quantity =71·37 inches, fell in 1811, and the minimum quantity =38·45, in 1803.

NEW COLLEGE, EDINBURGH,
June 18, 1849.

New Adamantine Mineral from Brazil.

M. Dufrenoy lately exhibited before the French Academy a specimen of a mineral from Brazil, which appears to be to the diamond what emery is to corundum, as stated by M. Elie de Beaumont. Among some specimens recently sent to the Ecole des Mines, by M. Hoffman, a dealer in minerals, were two which were stated to be hard enough to polish the diamond; and, in fact, the hardness of these specimens was found to be superior to that of the topaz.

This substance was analysed by M. Rivot, mining-engineer, who had at his disposal one large fragment weighing 65·760 grs., and several small pieces, weighing rather less than 0·50 gr.; the latter only were analysed. The large fragment appeared to come from the same alluvial formation as that in which the Brazilian diamonds occur. Its edges are rounded by long friction; but it has not the appearance of a rolled flint. It is of a slightly brownish dull black colour. Viewed with a glass, it appears riddled with small cavities separating very small, irregular laminæ, which are slightly translucent and iridescent. The brown colour is very unequally distributed throughout the mass. On one of the faces the cavities are linear, which gives it a fibrous aspect similar to obsidian. It cuts glass readily, and scratches quartz and topaz; its density is only 3·012. The small fragments subjected to analysis weighed, 0·444 gr., 0·410 gr, and 0·332 gr.; their densities were respectively 3·141, 3·416, and 3·255.

These numbers indicate great difference in the porosity of the specimens; they lead, however, to the conclusion, that the density of the substance is very nearly the same as that of the diamond. By means of long calcination at a bright-red heat in a covered crucible, the specimens were not altered; they retained their aspect, hardness, and weight; they do not, therefore, contain any substance volatilizable by calcination out of contact of the air. This result, certainly, does not prove the igneous origin of these diamonds, but renders improbable the idea expressed by M. Liebig, that diamonds are derived from the transformation of organic vegetable matter.

The three specimens were successively burned in pure oxygen gas in the apparatus employed by M. Dumas for the combustion of the diamond. The oxygen obtained from chlorate of potash was contained in a gasometer; it was dried and purified before it reached the combustion tube, by passing through two tubes containing sulphuric acid and pumice, and one tube with potash; employing this method with the precautions indicated by M. Dumas, 100 of the first specimen gave, carbon 96·84, ash 2·03; loss 1·13: second specimen gave, carbon 99·73, ash 0·24; loss 0·03: third specimen gave, carbon 99·87. ash 0·27; loss 0·36.

In the combustion of the first specimen, only one bulb-tube with potash was employed, so that a portion of the carbonic acid produced by the combustion was lost; but in the other two experiments, in which two bulb-tubes, containing potash, were used, the second increased in weight some centigrammes.

The last two analyses prove perfectly that the specimens are composed entirely of carbon and ash. The ash was yellowish; and in the first specimen it had retained the form of the diamond. When examined by the microscope, the ash appeared to be composed of ferruginous alumina and small transparent crystals, the form of which could not be ascertained.—(*L'Institut,* Mars 2, 1849; *Philosophical Magazine*, vol. xxxiv., 3d series, No. 230, May 1849, p. 397.)

Notice of some Plants which have flowered recently in the Royal Botanic Garden. By J. H. BALFOUR, M.D., Professor of Botany in the University of Edinburgh. (With a Plate of the Quassia amara.) Communicated by the Author.

QUASSIA AMARA, Linn. Spec. Plant. ed. Willd., tom. ii., p. 567. Linn. fil. Suppl., p. 235. Lamarck Illust., t. 434. Decandolle, Annales du Museum, xvii. 323; Prodromus I. 733. Ad. Jussieu, Mémoires du Museum, xii., tab. 27, No. 43. Hayne, Darstellung und Beschreibung der in der Arzneikunde gebraüchlichen Gewächse, ix. 14 (tab.).

Curt. Bot. Mag., pl. 497. Lodd. Bot. Cab., pl. 172.—Nat. Ord. Simarubaceæ.

In the April number of this Journal a description was given of the Quassia plant in the Botanic Garden, in so far as regards its stem, branches, leaves, and flower-buds. At the time the description was written, there seemed to be no prospect of any flowers expanding, for they fell off in the state of bud. By bending the branches, however, Mr M'Nab has succeeded in making the plant send out several recemes, the flowers of which have come to perfection, and I am thus enabled to add a description of the flower, along with a characteristic drawing.

Flowers of a scarlet colour, in terminal bracteated racemes. *Peduncle* terminal, about 2 inches in length, dark crimson, covered with small, acute, dark-coloured, hairy bracts, the lower ones empty, upper ones bearing each a pedicellate flower. *Pedicels* about $\frac{1}{10}$ of an inch in length, as long as the bracts, which are recurved at the apex; a contraction occurs where the flower is attached to the pedicel. Peduncles, pedicels, and bracts have scattered hairs. *Calyx* dark crimson, bibracteolate at the base; limb divided into five small, rounded, ovate segments, which are toothed at the margin. *Corolla* bright crimson, contorto-imbricate in æstivation, when fully developed still retaining a twisted appearance. *Petals* 5, with scattered hairs outside, more or less imbricated, and often slightly rolled in at the margin, rather more than an inch in length, ovate-lanceolate, blunt at the apex, curved at the lower part, where, by their apposition, they form a sort of sac. *Stamens* 10, longer than the corolla; filaments about $1\frac{1}{4}$ inch in length, of a pink colour, each with a white, scale-like, curved, hairy appendage at its base; anthers versatile, dithecal, lobes separated at the base, introrse, with longitudinal dehiscence; pollen trigonous, with 3 points where the intine protrudes. *Ovary* consisting of 5 united but easily separable carpels, supported on a large discoid gynophore, the lower part of which is adherent to the calycine tube. The 5 styles which proceed from the carpels are twisted together, and become blended so as to form at the upper part a single style, ending in a lobed and discoid blunt stigma; ovule solitary in each carpel, suspended, anatropal; embryo exalbuminous. *Fruit* (not perfect in the plant in the garden, and described therefore from a dried specimen communicated by Dr Christison) consists of five drupes spreading out horizontally from the gynophore, occasionally one or more are abortive; each drupe when dried is surrounded by a keel, which is very prominent on the upper side; epicarp dark-brown, with projecting reticulated veins. *Seed* suspended from the inner angle of the drupe. *Embryo* exalbuminous, cotyledons fleshy, radicle superior.

Explanation of the Drawing.

The drawing (Plate III.) has been executed by Mr James M'Nab, the uperintendent of the Botanic Garden.

1. Flowering branch, with impari-pinnate leaf and winged petiole.

2. Jointed leaf, with winged petiole and undivided blade. 3. Stamen, with hairy scale at its base. 4. Lateral view of a staminal scale. 5. Ovary, consisting of 5 carpels, seated on a large gynophore, with divided persistent calyx at the base, single style and lobed stigma.

CINNAMOMUM NITIDUM, *Nees ab Esenb.* Shining-leaved Cinnamon. Nat. Ord. Lauraceæ.—Enneandria Monogynia.

GENERIC CHARACTER.—Flores hermaphroditi vel polygami. Perianthium sexfidum, coriaceum, limbi parte superiore vel toto limbo a tubo cupuliformi deciduo. Stamina fertilia novem, triplici serie, quorum tria interna staminodiis binis sessilibus glanduliformibus ad basin stipata; antheræ ovatæ, sex exteriores introrsæ, tres interiores extrorsæ, omnes quadri-ocellatæ, valvulis totiden adscendentibus dehiscentes, locellis inferioribus magis lateralibus. Staminodia tria capitulo ovato in serie magis interiore. Ovarium uniloculare, uniovulatum. Stigma discoideum. Bacca monosperma, perianthii basi cupuliformi subsexfida stipata.—Arbores Indiæ Orientalis, ob corticem aromaticum celebres; foliis nervosis per paria approximatis vel suboppositis; floribus paniculatis, rariusve fasciculatis, exinvolucratis, gemmis nudis. N. ab E.

SPECIFIC CHARACTER.—Ramis teretibus glabris, foliis ovato-ellipticis basi apiceque subattenuato-obtusis, triplinerviis, obsolete venulosis, superioribus majoribus, paniculis subterminalibus axillaribusque, inferioribus sessilibus elongatis, floribus argenteo-sericeis, laciniis ellipticis medio deciduis. *Nees ab Esenbeck,* Syst. Laurin., p. 43. Plant. Officin. Suppl. Fasc. iv., tab. 8. *Wallich*, Plant. Asiat. rar., p. 73, No. 6.

The specimen in the Edinburgh Botanic Garden is a tree upwards of 20 feet high, and 7½ inches in circumference at its base. *Bark* has a slight taste of cinnamon; that of the trunk is of a greyish-brown colour; that of the young branches dark green. *Leaves* have a marked cinnamon flavour. They are all more or less broadly ovate, and attenuated towards the apex, blunt and slightly unequal at the base, usually placed alternately, but sometimes becoming opposite: *Petioles* about an inch in length, flattened and grooved on the upper surface: *Laminæ* subcoriaceous, varying from 2 to 6 inches in length, and from 1½ to 4 inches in breadth; dark green, shining and smooth on the upper surface, glaucous below, triplinerved, ribs prominent, lateral ribs vanishing towards the apex of the lamina, and giving off occasionally near the base subsidiary slender ribs, which only proceed for a short way up; sometimes an obscure rib runs along the outer margin of the leaves on each side; transverse veins somewhat arched and parallel, forming ultimately an angular net-work. *Panicles* terminal and lateral, cymose, somewhat corymbose, lax and spreading. *Rachis, peduncles, and pedicels* more or less hairy, becoming thickly covered towards their apex with short hoary pubescence. Primary divisions of the rachis (peduncles) diverge in pairs, bearing from 6 to 12 or more flowers. *Æstivation* imbricated. *Perianth* sulphur-yellow, 6-partite, pubescent, segments ovate, blunt at the apex (3 outer ones rather acute), concave on the inner surface, spreading when in flower. *Stamens* 9 fertile, the 6 outer in two rows, having introrse anthers, the 3 inner forming the third whorl, having extrorse anthers, and each bearing at the base of the filaments two very shortly-stalked yellow cordate-ovate glands. The fourth or innermost staminal whorl consists of 3 staminodia or abortive stamens, having yellow heads, which are triangular-cordate in front. *Filaments* and stalks of the glands and staminodia hairy. *Anthers* opening by 4 recurved valves. *Ovary* oblong, as long as the style. *Style* simple. *Stigma* capitate.

The plant has been in flower in the garden for 2 months. Mr M'Nab states that "the plant is growing freely in a mixture of rough loam and peat, about two parts of the former to one of the latter. It luxuriates in a warm, shady stovehouse, and requires a good deal of water, with frequent syringing amongst the leaves and branches. It may be increased by cuttings, covered with a bell-glass and placed in bottom heat." In Dr Neill's garden, Canonmills, there is a specimen of this plant twenty years old which has repeatedly flowered. The specimen figured as Cinnamomum nitidum by Hooker in Exotic Flora, vol. iii., p. 176, and in Hayne's work on Medical Botany, vol. xii., p. 22, as well as that preserved as Laurus nitida in the Hamiltonian Herbarium in the University of Edinburgh, appear to be C. eucalyptoides of Nees, which has more elliptical and not acuminated leaves. In the last-mentioned species the bark and leaves are said to have rather the taste of cloves, and the glands have distinct stalks. The present species resembles, in the form of its leaves, a variety of the true cinnamon, C. Zeylanicum.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY AND HYDROLOGY.

1. *Climate of Italy.*—M. Dureau de la Malle closes an elaborate investigation into the climate of ancient Italy, with the conclusion that the limits for different agricultural products were the same in the earlier as in more recent periods; and that, from the time of Augustus till the present, there has been no sensible modification of temperature, either as regards the months or years.

2. *Analysis of the Water of the Mediterranean off the Coast of France.*—M. J. Usiglio analysed the water taken from the foot of Mount St Clair, about 4000 metres from the port of Cette.

100 parts gave, chloride of sodium, 2·9424; bromide of sodium, 0·0556; chloride of potassium, 0·0505; chloride of magnesium, 0·3219; sulphate of magnesia, 0·2477; sulphate of lime, 0·1357; carbonate of lime, 0·0114; peroxide of iron, 0·0003; water, 96·2345 = 100·000.—(*Comptes Rendus*, October 1848.)

MINERALOGY.

3. *Copper of the Lake Superior Region.—(From a recent letter by C. T. Jackson.)*—The native copper mines of this region are truly wonderful, both for the quantities that are exposed in the mines, and the magnitude of the masses of native metal. Truly they are *copper veins.* I have seen the most noble lumps in this place, and one has lately been blown off in stopping the Cliff mine, Eagle River, that will weigh 50 tons. It is now cutting up into pieces of two or three tons weight, so that it may be sent to market. The supply furnished by that mine is as regular as it is in most mines furnishing ore. This spring the miners had 400 tons on hand, and they have sent down to Baltimore 600 tons at this time; and they estimate the amount of copper that they will ship at from 900 to 1000 tons before the close of navigation in November next. This mine has been wrought with proper energy, and will richly repay the owners. There are several other native copper mines here that are equally promising, and will produce well when wrought with proper energy and skill.

Copper Falls mine is an example, and is doing well. The north-west is another full of promise, and I have seen others which look very rich, but which are not yet opened deep enough to exhibit their contents. The shafts at the Cliff mine are 205 feet deep, and the hill above shews the vein in the face 213 feet higher, so that we know that the copper extends at least 418 feet. Those who were surprised that I recommended working mines for *native* copper, should come and see, and they would believe. The case is indeed a new one, and we watch with interest the results.

Native Silver is found mostly at and near the junction of the trap and sandstone where the veins end, not passing into the sandstone. —(*American Journal of Science and Arts*, vol. vii., Second Series, March 1849, p. 286.)

4. *Native Silver in Norway.*—It is reported in the Swedish official paper of the 27th October, that at the King's mine, at Kongsberg, two lumps of native silver, severally 238 and 436 pounds, were obtained within the preceding two months. This mine was offered for sale in London twenty years ago for £10,000, but failed of purchasers. It now brings to the Government more than this sum annually.

5. *The Arkansite*, according to the examination of Mr Whitney, is pure titanic acid, with only a trace of iron (and not a niobate, as inferred by Professor Shephard), and has the crystalline form and specific gravity of Brookite. His trials make the specific gravity 4·085. Its insolubility in acids is strong presumptive proof that it is not titanic acid in combination with a base, since all the known titanates are soluble in acids.—(*American Journal of Science and Arts*, vol. vii., No. 21, p. 433.

GEOLOGY.

6. *Movement of Heat in Terrestrial Strata of different Geological Natures. By M. Dove.*—In a work published in the Memoirs of the Academy of Berlin for 1844, on the relation of the changes of the temperature of the atmosphere and the development of plants, the author has endeavoured to determine to what changes of temperature a plant was subjected at different periods of the year. These investigations naturally divided themselves into two parts;—to what changes of temperature are the different parts of plants subjected which grow freely in the open air; and what temperatures act upon the roots which penetrate deeply into the earth. The first point could be determined pretty correctly, by a series of observations, carried on for a great number of years in the Botanic Garden of Chiswick, for the purpose of comparing the calorific phenomena presented by plants in the shade, with the temperatures indicated by plants exposed on all sides, in a place open to the whole influence of the sun, and of nocturnal radiation. With regard to the second part, the decennial series of observations on the heat of the ground at Brussels, presented valuable materials; but as the ground had always been of the same nature, we could obtain from these only the difference between the shade and the radiation, and not the modifications which might arise, in formations of diverse natures, from their different conducting power, their capacity of radiation and their specific heat, relatively to the movement of the heat in the interior of variable strata. As these differences are by no means unimportant, a comparison has been established between the observations of Heidelberg and those of Schwetzinger, the former of which were made on a compact clayey soil, and the second, on a light sandy formation, but which was not above five feet deep, and presented great irregularities. The blanks may be filled up by the calculation of observations which have been made, since 1837, at the depth of 3, 6, 12, and 24 French feet, at Edinburgh, in the trap of the Calton Hill, the sandstone of the coal-formation at Craigleith, and the sand of the Experimental Garden.

It follows from these calculations, that the extent of the changes, whether periodical or non-periodical, is unimportant or insensible in the trap, more considerable in the sand, and reaches its maximum in the sandstone; so that the further the roots of a plant penetrate into the soil, the more it lives in conditions approaching these of a maritime climate; and, on the other hand, when the roots are of equal depth, the same effect becomes so much the more sensible as the roots penetrate into a soil of inferior conducting powers. Whence it evidently follows, that the geological nature of a formation is important for the development of plants, not only in a chemical *point of view*, but also in a physical.—(*L'Institut*, No. 711, p. 169.)

ZOOLOGY.

7. *The Dodo arranged with the Grallæ.*—Mr Brandt, at present engaged with an extensive memoir on aquatic birds, has had his attention drawn aside from that subject by the receipt of interesting details regarding the dodo, furnished to him by Dr Hamel and the Directors of the Museum of Natural History of Copenhagen. This remarkable bird, a former inhabitant of the Mauritius, but extinct for 200 years, is placed by Mr Brandt among the Grallæ, and he announces the discovery of certain osselets peculiar to the cranium of the Grallæ, which have furnished him with new characters for the classification of that order, so rich in species.

8. *The Fossil Rhinoceros of Siberia and the Mammoth Natives of the countries where their Fossil Remains are found.*—Mr Brandt, at the request of Humboldt, has communicated to the Petersburg Imperial Academy the results of his microscopical examination of the remains of food found in the hollows of the teeth of the antediluvian rhinoceros, of which the Academy possesses a complete cranium, still covered with skin. According to his researches, it appears that this species of rhinoceros fed on the leaves and fruit of coniferous plants; hence there is no reason for supposing that the great fossil animals found buried in arctic countries have ever lived in a tropical one. The bushy hair with which they were clothed, and the examples of mammoths found in an upright position, rather incline him to the opinion that these species lived in the countries and climate where their fossil remains are now found, than to have recourse to the hypothesis of a sudden change of temperature of the climate, or to the opinion of the transportation, by inundation, of their remains from a far distant country.

9. *What becomes of the Skeletons of Wild Animals after death?*—The following interesting fact is related by the Count de Montlosier, in his Memoirs, published in Paris. It is, as far as we have found, perfectly new, and the general observation, of which the fact is illustrative (that of the extreme rarity of meeting with any instances of wild animals dying of what is called a natural death), has been less attended to and investigated than almost any other that could be named, though it is one of singular interest, and of great importance as connected with the study of natural history. The Count de Montlosier says, that his thoughts had long been occupied on the manner in which animals living in a natural state,—hares, rabbits, and game of all kinds,—met their death, and what became of their remains; and he had repeatedly promised large rewards to gamekeepers and others, who would procure him any animal in that state, but had never been able to meet with one. He then adds, that, having long made himself acquainted with nearly all the caves and caverns in the mountains neighbouring the spot where he resided, there was one which had hitherto remained unexamined even by himself, and was quite unknown to every one else; which he had

neglected to examine minutely, on account of the extremely small opening to it, which prevented entrance, except by creeping on the hands and knees, and even then allowed it with great difficulty. One day, however, he succeeded in getting in, and his surprise was great on finding himself in a large vaulted cavern, so high that his hand could not reach the top of it. He advanced a little way, but finding it perfectly dark, and that he was in danger of losing sight of the orifice by which he had entered, he immediately got out again, and went in search of light and assistance. On returning, and making their way again into the cavern, they discovered that it contained a vast number of skeletons, which appeared to be those of hares or rabbits. They were extended on the ground, all placed in a nearly similar manner, and shewing at once that they could not have been brought there by any beasts of prey, as the bones were all perfect, and even the cartilages were preserved; and on of some of them there were even portions of the hair and flesh not decayed.

10. *Miraculous Blood spots on Human Food.*—Under the influence of certain circumstances, of which it is difficult, if not impossible, now to form any precise idea, there has appeared upon bread, and food of other kinds, spots of a vivid red colour, closely resembling drops of blood. During the siege of Tyre, Alexander was alarmed by the appearance of bloody spots on the soldiers' bread. At a period nearer our own age, in 1510, similar stains were seen upon the consecrated wafers, and thirty-eight unfortunate Jews were accused of having caused, by their sorceries, this phenomenon, and suffered death, by burning, for their supposed sacrilege. In 1819, similar kinds of red spots appeared amongst the inhabitants of Padua and its environs. At the commencement of the month of August in that year, a farmer of Segnaro, named Pittarello, was frightened by seeing drops of blood sprinkled upon his porridge, made of the maize which grew in the neighbourhood of his village. His alarm was greatly increased, when, for many days following, he saw the same red spots appear on all his food—new bread, rice, veal, fish, boiled and roast fowls. The *curé* was appealed to, that he might exercise his sacred functions to expel the evil spirit which produced these alarming appearances; but prayers were ineffectual, and the neighbours of the unfortunate Pittarello supposed that he was under a celestial malediction. Incited by curiosity, a large number of persons went to Segnaro, and a commission was eventually named to investigate the nature and causes of this phenomenon. M. Sette was appointed to this task. On examining under the microscope these miraculous red spots, he discovered that they were formed by myriads of small bodies, which appeared to be *microscopic fungi*, and to which he gave the name of *zaogalactina imetropha*. He succeeded in propagating these minute organic productions, and in a memoir published at Venice in 1824, he gives a detailed history of them. During the year 1848, the same phenomenon appeared at Berlin, and fixed the attention of Ehrenberg. This celebrated micro-

grapher has closely studied these red spots, and he believes them to be, not as M. Sette supposes, microscopic fungi, but animalculæ of inferior degree, a monade to which he has given the name of *Monas prodigiosa,* on account of their extreme smallness. These little beings appear as corpuscles, almost round, of one three-thousandth to one eight-thousandth of a line in length; transparent when separately examined, but in a mass of the colour of blood. M. Ehrenberg calculates, that in the space of a cubic inch there are from 46,656,000,000,000 to 884,836,000,000,000 of these monades.—(*Medical Times*, No. 497, vol. xix., April 1849.)

11. *The Oyster.*—M. de Quatrefages has recently ascertained that, contrary to the common opinion, the sexes are separate in the oysters. M. Blanchard's observations confirm those of M. de Quatrefages. In his investigations into the Nervous System of Mollusca, he has had occasion to examine a great number of these animals, and in the proper season he has always found the eggs and the spermatozoa isolated in different individuals.—(*American Journal of Science and Arts*, vol. vii., No. 21, p. 437.)

12. *Process of preparing the Spawning Beds by Fishes.*—The process of preparing the spawning beds is curious. The two fish come up together to a convenient place, shallow and gravelly. Here they commence digging a trench across the stream, sometimes making it several inches deep. In this the female deposits her eggs or ova; and she having left the bed, the male takes her place and deposits his spawn on the ova of the female. The difference may be, perhaps, easily exemplified by the *soft* and hard roe of a herring,—the former being that of the male, and without this the hard roe or ova of the female fish would be barren. When the male has performed his share of the work, they both make a fresh trench immediately above the former one, thus covering up the spawn in the first trench with the gravel taken out of the second. The same process is repeated till the whole of their spawn is deposited, when the fish gradually work their way down to the salt water to recruit their lost strength and energy.

The spawn is thus left to be hatched in due time, but is sometimes destroyed by floods, which bury it too deep, or sweep it entirely away; at other times it is destroyed by want of water, a dry season reducing the river to so small a size as to leave the beds exposed to the air. The time required to hatch the eggs depends much on the state of the weather; in warm seasons they are hatched much quicker than in cold. The details I have here given are very imperfect; but, perhaps, they may induce those interested in the subject to read a little work published by Mr Young, the result of his observations and experience for many years.—(*Field Notes and Tour in Sutherland, by Charles St John,* vol. i. p. 55.)

BOTANY.

13. *The Distribution of Flowers in a Garden.*—Amongst the

easures presented to us by the culture of flowering plants, there e few that exceed what we experience from the sight of a multi-de of flowers, varying in their colour, form, and size, and in their rrangement upon the stem that supports them. It is probably wing to the admiration bestowed individually upon each, and to the ttachment bestowed upon them in consequence of the great care they ave required, that care has hitherto not been taken to arrange them n such a manner as to produce the best possible effect upon the eye, not only separately, but collectively. Nothing, therefore, is more common than a defect of proportion observed in the manner in which flowers of the same colour are made to recur in a garden. At one time the eye sees nothing but blue or white, at another it is dazzled by yellow scattered around in profusion; the evil effect of a predominating colour may be further augmented when the flowers are of approximating, but still different shades of colour. For instance, in the spring, we meet with the jonquil of a brilliant yellow, side by side with the pale yellow of the narcissus; in the autumn the Indian pink may be seen next to the China rose and the aster, and dahlias of different red grouped together, &c. Approximations like these produce upon the eye of a person accustomed to judge of the effects of the contrast of colours, sensations that are quite as disagreeable as those experienced by the ear of the musician, when struck by discordant sounds.

The principal rule to be observed in the arrangement of flowers is to place the blue next to the orange, and the violet next to the yellow, whilst red and pink flowers are never seen to greater advantage than when surrounded by verdure and by white flowers; the latter may also be advantageously dispersed among groups formed of blue and orange, and of violet and yellow flowers. For although a clump of white flowers may produce but little effect when seen apart, it cannot be denied that the same flowers must be considered as indispensable to the adornment of a garden when they are seen suitably distributed amongst groups of flowers whose colours have been assorted according to the law of contrast; it will be observed by those who may be desirous of putting in practice the precepts we have been inculcating, that there are periods of the horticultural year when white flowers are not sufficiently multiplied by cultivation to enable us to derive the greatest possible advantage from the flora of our gardens. I will further add, that plants, whose flowers are to produce a contrast, should be of the same size, and, in many cases, the colour of the sand or gravel composing the ground of the walks or beds of a garden, may be made to conduce to the general effect.

In laying down the preceding rules, I do not pretend to assert that an arrangement of colours, different from those mentioned may not please the eye; but I mean to say that, in adhering to them, we may always be certain of producing assemblages of colour conformable to good taste, whilst we should not be equally sure of success in *making other arrangements*. I shall, however, revert to this *point.*

I will reserve for a special article the consideration of the number of plants in flower at the same time, which admit of being grouped together, and of those details of execution which would here be out of place. I must, however, reply to the objection that might be made, *that the green of the leaves, which serves, as it were, for a ground for the flowers, destroys the effect of the contrast of the latter.* Such, however, is not the case, and, to prove this, it is only necessary to fix on a screen of green silk two kinds of flowers, conformably to the arrangement of the coloured stripes, and to look at them at the distance of some ten paces. This admits of a very simple explanation; for as soon as the eye distinctly and simultaneously sees two colours, the attention is so rivetted that contiguous objects, especially when on a receding plane, and where they are of a sombre colour, and present themselves in a confused manner to the sight, produce but a very feeble impression.—(*Chemical Reports and Memoirs of the Cavendish Society*, p. 207.)

14. *The Nutmeg Tree* (*Myristica officinalis*).—Banda can furnish annually 500,000 lb. of nutmegs and 150,000 lb. of mace: this latter is not, as some persons suppose, the flower of the nutmeg, but the immediate internal cover of the brown shining shell, covering the kernel, which is the nutmeg; it is found as a beautifully reticulated scarlet arillus between these and the husk or exterior green skin.

The tree which furnishes these two productions, is one of the most agreeable to the eye, at least I thought so, when, for the first time, I saw a number loaded with fruit at Pondokgede, where they border the large walks of the magnificent garden belonging to the Nestor of our eastern possessions, the worthy M. W. Engelhard. The nutmeg tree attains a height of thirty-five to forty feet; it has some resemblance to our European pear-tree; its leaf is of a deep and shining green. Commencing to bear fruit about its ninth year, the tree produces, during more than half a century, if care be taken to shelter it properly, which is done at Banda, by placing it in plantations of canari trees, or of wild nutmegs, which the inhabitants call *pala boeig*; these have the same leaf and flower, but they give no fruit.

When the flower of the nutmeg falls, it is replaced by the nut; this requires several months to attain maturity, when it is of the size and the form of an apricot; its skin, of a yellowish-green, opens and displays the nutmeg, covered with its mace, of a beautiful red colour. The average annual produce of a tree is calculated at 5 or 6 lb. of nuts; there are some, however, which give from 15 to 20 lb. Although the nutmeg bears during the greater part of the year, the principal crop is in August, and a second, in November and December. These crops are liable to turn out more or less good. Good nuts are sometimes ill provided with mace, and often, on the contrary, very inferior nuts are accompanied by a superior mace.

The nuts, carefully withdrawn from their green exterior skin, and from the mace, are exposed to the smoke during two or three months upon frames or hurdles, in buildings constructed for the purpose

(*Kombuisen*), and then deprived of a last interior and very hard shell, an operation which is called *afklopping van de noot*, in order speedily to be steeped in lime mixed with sea-water. This method of preparing the produce requires the greatest precautions, for it is very delicate, and very easily deteriorated. The mace ought to be thoroughly dried, but by the sun or wind; sometimes the planters, when the season is humid, secretely avail themselves of the smoking frames (*rook pavia pavias*) to accelerate the operation; but then the mace acquires an inferior colour, and sweats more slowly, when it is exposed during the voyage to the heat at the bottom of the hold.—(*Journal of the Indian Archipelago and Eastern Asia*, vol. iii., No. 1, p. 12.)

15. *Cloves of Amboyna.*—But that which, above all, has made Amboyna so precious, is the culture of the clove (the flower-buds of the *Caryophyllus aromaticus*).

In an average year, the crop of cloves may be reckoned at 250,000 or 300,000 lb. There are years, like those of 1819 and 1820, when this quantity has been much surpassed; but then in others, the crops have been less; in 1821, it did not amount to 100,000 lb.—(*The Journal of the Indian Archipelago and Eastern Asia*, vol. iii., No. 1, p. 10.)

NEW PUBLICATIONS.

1. Kosmos. By Alexander Von Humboldt. Translated by Colonel Sabine, F.R.S. Fourth edition, 2 vols. Longmans, and John Murray, London, 1849. *The cheapest, most correct, and best translation of the renowned work "Kosmos" we have seen.*

2. A Manual of Botany, being an Introduction to the Study of the Structure, Physiology, and Classification of Plants. By John Hutton Balfour, M.D., Professor of Botany in the University of Edinburgh. Illustrated by numerous Woodcuts. One vol. 8vo. Griffin & Co., London. Glasgow: Griffin & Co., 1849. *Although there is a great deficiency of elementary works in Zoology in this country, we rejoice, as botanists, that we possess such Botanical manuals as those of Jussieu, Schleiden, Lindley, Henfrey; and we now add the recently-published excellent Manual of Professor Balfour, which is equal, and in some respects superior, to the other manuals in our language at present in extensive circulation.*

3. The Elements of Botany. By M. Advien de Jussieu, Member of the Institute of France, &c., &c. Translated by J. H. Wilson, F.L.S., &c. One vol., pp. 750. Van Voorst, London, 1849. *We had much to say of this classical work, but the limits of our Journal do not admit of detail. We can only remark that the translation is good, the additions well selected, the numerous engraved illustrations very creditable to the artist, and the typography beautiful.*

4. Introduction to Meteorology. By D. P. Thomson, M.D. One vol. 8vo, pp. 487. Blackwoods, Edinburgh and London, 1849. *This meritorious compilation we recommend to the attention of students of Meteorology. The industrious author has made ample use of the Lec-*

tures on this branch of Natural History—and hence its fulness of detail.

5. Principles of Scientific Botany; or Botany as an Inductive Science. By Dr J. M. Schleiden, Professor of Botany in the University of Jena. Translated by E. Lankester, M.D., F.R.S., F.L.S., &c. One vol. 8vo, pp. 616. Longman, Brown, Green, and Longmans, London, 1849. *We congratulate our readers on the appearance of an English edition of this remarkable work, by a gentleman so capable to do full justice to it as Dr Lankester. It cannot fail to interest deeply all true lovers of Botanical Science, and we believe it will be considered a valuable addition to our Botanical literature.*

6. The Isle of Man: its History, Physical, Ecclesiastical, Civil, and Legendary. By the Rev. J. G. Cumming, M.A., F.G.S., Vice-Principal of King William's College, Castletown. 8vo, pp. 376, with numerous Illustrations. J. Van Voorst, London, 1848. *Mr Cumming's interesting volume gives the most satisfactory and comprehensive view of the statistics and geology of the Isle of Man hitherto published.*

7. Histoire des Progress de la Geologie de 1834 à 1845, par Le Vicomte d'Archiac. Tome Premiere. Cosmogonie, Geogonie, Physique du Globe, Geographie Physique, Terrain Modern. Paris, 1847. *Count d'Archiac's very useful work, publishing by the Geological Society of Paris, and under the sanction of the Minister of Public Instruction, so well begun, we trust will be continued, and without interruption, notwithstanding the present disordered political state of Paris.*

8. Explication de la Carte Geologique de la France, redigée par MM. Dufrenoy et Elie de Beaumont. Tome 2. 2to, pp. 813. Paris, 1848. *The present volume of the celebrated Geological Survey of France, like that already published, is remarkable for its rich display of facts illustrative of the varied geognostical and œconomical relations of the rock formations of that empire. This volume is dedicated to the Trias system, including the variegated sandstone, shell limestone, and variegated marls, and the Jura system, consequently including the Lias, and the lower, middle, and upper Oolite. These systems are illustrated by 105 interesting sections and plans. It is announced that the third volume will contain an account of the remaining Neptunian formations; and that a separate volume will be published, with descriptions and figures of the Fossil Molluscs characteristic of the different fossiliferous deposits of France.*

9. Lectures on the Study of Chemistry, in connection with the Atmosphere, the Earth, and the Ocean: and Discourses on Agriculture; with Introductions on the present state of the West Indies, and on the Agricultural Societies of Barbados. By John Davy, M.D., Inspector-General of Army Hospitals. Longman, Brown, Green, and Longmans. London, 1849. *This interesting little volume, worthy the reputation of its distinguished author, cannot but prove both instructive and acceptable to the numerous class of readers for which it is intended.*

10. Manual of Mineralogy; or the Natural History of the Mineral Kingdom. By James Nicol, F.R.S., Assistant-Secretary to the Geological Society of London. 8vo, pp. 576. Adam and Charles Black, Edinburgh, and Longmans, London. *Mr Nicol, in his Manual, one of the best elementary works on Mineralogy lately published in our language, arranges minerals according to the system of the celebrated Prussian*

mineralogist Weiss, thus following the example of Hartmann, in his System of Mineralogy. The method is good, in so far as the general chemistry of minerals is concerned, but the want of physical characteristics of classes, orders, families, and genera, renders the work less immediately useful than it would otherwise be, to the young mineralogist, who, owing to these omissions, is left to the uncertain mode of discovering the place of the species in the system by an appeal to the index. In the next edition of Mr Nicol's Manual, we would recommend him to supply this deficiency, which we know he is fully competent to do.

11. Passages in the History of Geology. By Professor Ramsay, of University College, London. *These pages contain a short, judicious, clearly-written, and well-timed sketch of the progress of Geology, up to the time of the celebrated Hutton and Playfair of Edinburgh, authors of the present generally-adopted speculative views in Geology.*

12. Tour in Sutherlandshire. By Charles St John, Esq. 2 vols. 8vo. John Murray, London, 1849. *These amusing volumes, a continuation of Mr St John's former work, will be found equal to it, in usefulness and interest, especially to those who may visit the romantic wilds and picturesque coasts of the remote Sutherland.*

List of Patents granted for Scotland from 22*d March to* 22*d June* 1849.

1. To Charles-Henry Paris, of Paris, in the republic of France, manufacturer, "improvements in preventing the oxidation of iron," being a communication from his brother, Charles-Emile Paris, residing abroad.—26th March 1849.

2. To William-Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in machinery for hulling and polishing rice and other grain or seeds," being a communication from a foreigner residing abroad.—26th March 1849.

3. To James Fletcher, of Salford, in the county of Lancaster, manager at the works of Messrs William Collier and Company, of Salford aforesaid, machinists and tool-makers; and Thomas Fuller, of Salford aforesaid, machinist and tool-maker, a partner in the said firm, "certain improvements in machinery, tools, or apparatus for turning, boring, planing, and cutting metal and other materials."—26th March 1849.

4. To Walter Neilson, of Hyde Park Street, in the city of Glasgow, North Britain, engineer, "a certain improvement or improvements in locomotive engines."—27th March 1849.

5. To Jean-Adolphe Carteron, of Paris, in the republic of France, now of the Haymarket, in the county of Middlesex, chemist, "certain improvements in dyeing."—27th March 1849.

6. To David Henderson, of the London Works, in the parish and county of Renfrew, Scotland, engineer, "improvements in the manufacture of metal-castings."—29th March 1849.

7. William Longmaid, of Beaumont Square, in the county of Middlesex, gentleman, "improvements in treating the oxides of iron, and in obtaining various products therefrom."—4th April 1849.

8. Francis Hay-Thomson, of Hope Street, in the city of Glasgow, North Britain, doctor of medicine, "an improvement or improvements in smelting copper or other ores."—11th April 1849.

9. To Clemence-Augustus Kurtz, of Wandsworth, in the county of Surrey, gentleman, "certain improvements in looms for weaving," being a communication from a foreigner residing abroad.—11th April 1849.

10. To Barthelenny Thimounier-Aine, of Amplepuis Department Du Rhone, in the republic of France, engineer, "improvements in machinery for sewing, embroidering, and for making cords or plats."—11th April 1849.

11. To Arthur Dunn, of Dalston, chemist, "improvements in ascertaining and indicating the temperature and pressure of fluids."—13th April 1849.

12. To Alfred-Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the County of Middlesex, mechanical draughtsman, "improvements in the manufacture of piled fabrics," being a communication from a foreigner residing abroad.—13th April 1849.

13. To Jeremiah Brown, of Kingswinford, in the county of Stafford, roll-turner, "certain improvements in rolls and machinery used in the manufacture of iron, also in rolls and machinery for shaping or fashioning iron for various purposes."—13th April 1849.

14. William M'Bride jun., of Sligo, in the kingdom of Ireland, but now of Havre, in the republic of France, merchant, "improvements in the apparatus and process for converting salt water into fresh water, and in oxygenating water," being a communication from abroad.—16th April 1849.

15. To John Ruthven, Engineer, Edinburgh, Scotland, "improvements in preserving lives and property from water and fire, and in producing pressure for various useful purposes."—17th April 1849.

16. To William-Henry Balmain, and Edward-Andrew Parnell, both of St Andrews, in the county of Lancaster, manufacturing chemists, "improvements in the manufacture of glass, and in the preparation of certain materials to be used therein, parts of which improvements are also applicable to the manufacture of alkalies."—17th April 1849.

17. To Stephen White, of Victoria Place, Bury, New Road, Manchester, in the county of Lancaster, gas-engineer, "improvements in the manufacture of gases, and in the application thereof to the purposes of heating, and consuming smoke; also, improvements in furnaces, for economizing heat, and an apparatus for the consumption of gases."—19th April 1849.

18. To Loren H'jorth, of Jewry Street, Aldgate, in the city of London, "certain improvements in the use of electro-magnetism, and its application as a motive power; and also other improvements in its application generally by engines, ships, and railways."—20th April 1849.

19. To James Hart, of Bermondsey Square, engineer, "improvements in machinery for manufacturing bricks and tiles, parts of which machinery are applicable to moulding other substances."—13th April 1849.

20. To Charles Alexander Broquette, of Rue Neuve St Nicholas, St Martin, in the republic of France, chemist, "improvements in printing and dyeing fibrous and other materials."—20th April 1849.

21. To **Meyer Jacobs**, of Spittalfields, Middlesex, "certain improvements in the manufacture, stamping, and treatment generally, of woven fabrics of all kinds."—25th April 1849.

22. To **James Roose**, of Darleston, in the county of Stafford, tube-manufacturer, and **William Haden-Richardson** the younger, of the same place, tube-manufacturer, "improvements in the manufacture of tubing."—30th April 1849.

23. To **Robert Oxland**, of Plymouth, chemist, and **John Oxland** of the same place, chemist, "improvements in the manufacture of sugar."—4th May 1849.

24. To **Frederick Steiner**, of Hyndburn, near Accrington in the county of Lancaster, turkey-red dyer, "improved processes and apparatus to be used in the turkey-red dye, on cotton and its fabrics."—7th May 1849.

25. To **John Dalton**, of Hollingworth, in the county of Chester, calico-printer, "a certain improvement or certain improvements in printing calicoes and other surfaces."—9th May 1849.

26. To **Alexander Munkittrick**, of Manchester, in the county of Lancaster, merchant, "an improved corporation of matter, which is applicable, as a substitute for oil, to the lubrication of machinery, and for other purposes," which has been communicated to him by a foreigner residing abroad.—10th May 1849.

27. To **James Anderson**, of Abbotsford Place, in the city of Glasgow, North Britain, starch-manufacturer, "a certain improved mode of separating different qualities of potatoes and other vegetables."—11th May 1849.

28. To **Alexander Swan**, of Kirkcaldy, in the county of Fife, manufacturer, "improvements in heating apparatus, and in applying hot and warm air to manufacturing and other purposes, where the same are required."—14th May 1849.

29. To **Samuel Adams**, of West Bromwich, in the county of Stafford, organist, "improvements in mills for grinding."—16th May 1849.

30. To **Rees Reece**, of St John Street, Smithfield, and **Astley Paston-Price**, of Margate, in the county of Kent, chemists, "improvements in the manufacture and refining of sugar, or saccharine matters."—21st May 1849.

31. To **Daniel Miller**, civil engineer, of No. 186 St George's Road, in the city of Glasgow, in Scotland, "certain improvements in the mode of drawing ships up an inclined plane out of water; for which mode a patent was granted to the late Thomas Morton, of Leith, shipbuilder, on the 23d day of March 1819, and which mode has been commonly known as 'Morton's Slip.'"—21st May 1849.

32. To **Alphonse Garnier**, of Paris, in the republic of France, but now of South Street, Finsbury, in the county of Middlesex, merchant, "certain improvements in extracting and preparing colouring matter from orchil," being a communication from a foreigner residing abroad.—21st May 1849.

33. To **Moses Poole**, of the Patent Office, London, gentleman, "improvements in apparatus for drawing fluids from the human or animal body," being a communication from a certain foreigner residing abroad.—23d May 1849.

34. To **William Newton**, of the Office for Patents, 66 Chancery

Lane, in the county of Middlesex, civil engineer, "improvements in the Jacquard machine," being a communication from a foreigner residing abroad.—28th May 1849.

35. To Henry Vint, of St Mary's Lodge, Colchester, in the county of Essex, "improvements in propelling ships and other vessels."—29th May 1849.

36. To Malcolm Macfarlane, of Thistle Street, in the city of Glasgow, North Britain, coppersmith, "certain improvements in machinery, or apparatus for the drying and finishing of woven fabrics."—29th May 1849.

37. To Elijah Slack, of Orchard Street, in the burgh of Renfrew, North Britain, gum-manufacturer, "an improvement or improvements in the preparation of materials to be used in the manufacture of textile fabrics."—31st May 1849.

38. To Edward Buckler, of the city of London, merchant, "improvements in the manufacture of boots and shoes, also applicable to other purposes," being a communication from a foreigner residing abroad. —5th June 1849.

39. Jacques Hulot, of Rue St Joseph, Paris, in the republic of France, manufacturer of fabrics, "improvements in the manufacture of the fronts of shirts."—7th June 1849.

40. To Thomas Greenwood, of Goodman Fields, in the city of London, sugar-refiner, and Frederick Parker, of New Gravel Lane, Shadwell, animal charcoal-manufacturer, "improvements in filtering syrups and other liquors."—8th June 1849.

41. William Tait, Ironside, in the county of Warwick, printer and bookseller, "an improved method or methods of producing outlines on paper, pasteboard, parchment, papier mâché, and other like fabrics."—8th June 1849.

42. George Simpson, of Buchanan Street, in the city of Glasgow, North Britain, civil and mining engineer, "a certain improvement or improvements in the machinery, apparatus, or means of raising, lowering, supporting, moving, or transporting heavy bodies, such improvements being applicable to various useful purposes."—11th June 1849.

43. To Joseph Harrison, of Blackburn, in the county of Lancaster, machine-maker, "certain improvements in and applicable to looms for weaving."—11th June 1849.

44. To William Gratrix, of Salford, in the county of Lancaster, bleacher and dyer, "certain improvements in the method or process of drying and finishing woven and other fabrics, and in the machinery or apparatus for performing the same, parts of which improvements are applicable to stretching woven fabrics."—12th June 1849.

45. To Osgood Field, of London, merchant, "improvements in anchors," being a communication from a foreigner residing abroad.—14th May 1849.

46. To Robert Nelson-Collins, of Oxford Court, Cannon Street, in the city of London, wholesale druggist, "certain improved compounds to be used for the prevention of injury to health under certain circumstances."—14th June 1849.

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PHILOSOPHICAL JOURNAL.

Biographical Sketch of James Cowles Prichard, M.D., F.R.S., Corresponding Member of the Institute of France, &c., late President of the Ethnological Society, and Author of "Researches into the Physical History of Man." By THOMAS HODGKIN, M.D.*

JAMES COWLES PRICHARD was born on the 11th of 2d month (Feb.) 1786, at Ross, in Herefordshire, where his family had resided for several generations. His parents were members of the Society of Friends, and in its principles he was himself educated. After the usual preliminary school education he made choice of the medical profession, in which he was instructed in the London Hospitals, and afterwards in the University of Edinburgh, where he took his medical degree; the subject of his Thesis being the Physical History of Man. In 1810 he settled at Bristol as a physician. There, towards the close of the year 1813, he brought out the first edition of his celebrated work, "On the Physical History of Man." The views which he had at that time adopted, and the scope embraced by this work, the extension of which in subsequent editions occupied so large a portion of his attention, and justly procured him universal reputation, cannot be better stated that in the Doctor's own words:—

"The nature and causes of the physical diversities which characterize different races of men, though a curious and interesting subject of inquiry, is one which has rarely engaged the notice of writers of our own country. The few English authors who have treated of it, at least those who have entered into the investigation on physiological grounds, have, for the most part, maintained the opinion, that there exist in mankind several distinct species. A considerable and very respectable class of foreign writers, at the head of whom we reckon Buffon and Blumenbach, have given their suffrages on the contrary side of this question, and have entered more diffusely into the proof of the doctrine they advocate.

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* Read at the Meeting of the Ethnological Society, on the 28th February 1849.

"My attention was strongly excited to this inquiry many years ago, by happening to hear the truth of the Mosaic records implicated in it, and denied, on the alleged impossibility of reconciling the history contained in them with the phenomena of nature, and particularly with the diversified characters of the several races of men. The arguments of those who assert that these races constitute distinct species appeared to me at first irresistible, and I found no satisfactory proof in the vague and conjectural reasonings by which the opposite opinion has generally been defended. I was at last convinced that most of the theories current concerning the effects of climate and other modifying causes are in great part hypothetical, and irreconcilable with facts that cannot be disputed.

* * * * * *

"In the course of this essay I have maintained the opinion, that all mankind constitute but one race, or proceed from a single family, but I am far from wishing to interest any religious predilections in favour of my conclusions. On the contrary, I am ready to admit, and shall be glad to believe, if it can be made to appear, that the truth of the Scriptures is not involved in the decision of this question. I have made no reference to the writings of Moses, except with relation to events concerning which the authority of those most ancient records may be received as common historical testimony; being aware that one class of persons would refuse to admit any such appeal, and that others would rather wish to see the points in dispute established on distinct and independent grounds."

In this work Dr Prichard set forth the differences of colour, hair, stature, and form, and examined the value of each as an evidence of difference of race; and inferred from the *occurrence* of these and similar differences, where identity of race could not be doubted, that they must not be received as evidences against the unity of our species. He successfully combated the old opinion that the influence of the sun, continued through several generations, has produced the blackness of the Negro, and adduced instances in proof of the continuance of black or brown and the white complexion through numerous generations, in almost every latitude and climate. He inquired into the production and permanency of varieties in man and in inferior animals; examined some of the causes which may tend to produce them; and following up an idea adopted by John Hunter, that cultivation is a powerful cause of producing variety, and of lowering the intensity of colour in animals and plants, he makes the suggestion, that civilization has been the operative cause which has produced the white varieties of the human species, of which he supposed that the first pair were black. He related many curious facts collected from several parts of the globe in support of this bold and ingenious theory, the announcement of which excited both surprise and interest. Though the Doctor ventured to offer this conjecture, the work was throughout an appeal to fact and evidence: and not satisfied with merely inferring that resemblance in form, colour, language, and habits, are proofs of a community of origin amongst the inhabitants of distant islands, he adduced the instances of canoes with their crews having lost their way, and being conveyed by winds or currents to a distance of hundreds of miles across the ocean. The work contains a description of the known varieties of man, in which the author adopted the division proposed by Blumenbach, and exhibited a great amount of research in the number of authors from whom his descriptions were collected. Even at this early period of the author's researches, a large amount of labour and erudition were devoted to the ancient Egyptians and Hindoos.

About thirteen years intervened between the publication of the first and second editions of the Doctor's work; and as his growing celebrity as a physician had in the mean time raised him to eminence in his profession, it may not be amiss here to make a digression from his history as an ethnologist, in order to speak of him as a medical man, in which character he *would have been* distinguished had he written nothing upon ethnology. It has already been stated that Dr Prichard did not embrace the profession of medicine from any strong and early predilection. But what is of far greater importance to the study of the wide range of subjects which the science of medicine embraces, he brought to it that accurate observation which is the result of habitual exercise; and that aptitude for continued and varied study, which springs from the union of talent with early education, and is the surest preparation for sound professional knowledge, and safe and successful practice. And I may here be allowed to remark that nothing is more absurd than the vulgar error, that there may be an intuitive knowledge and natural gift which of themselves confer on their possessors a marvellous skill in the healing art. Dr Prichard applied himself with as much zeal to the practice as he had done to the study of his profession. He established a dispensary. He became physician to some of the principal Medical Institutions of Bristol. He had not only a large practice in his own neighbourhood, but was often called to distant consultations. Notwithstanding the engrossing nature of these occupations, he found time to prepare and deliver lectures on Physiology and Medicine, and wrote an essay on Fever and one on Epilepsy, and subsequently a larger work on Nervous Diseases.

Amongst the patients who came under the Doctor's care in public practice were the inmates of a lunatic asylum; and combining the results of his own observation and experience with that laborious research which he was accustomed to employ on all the subjects to which he directed his attention, he was enabled to produce an excellent treatise on Insanity, which was first published as one of the articles which he contributed to the " Encyclopædia of Practical Medicine."

Notwithstanding his numerous avocations, Dr Prichard continued his literary and scientific studies; yet many of these had more or less a bearing upon his favourite subject—the History of Man. He acquired the German language, in which so many profound works on philology and history are composed; and as an excercise, he prepared and published, in conjunction with his friend W. Tothill, a translation of Müller's General History. He wrote an article on the Mithridates of

Adelung. He continued his researches on Egyptian mythology and history, in which he investigated their relations to those of India. He contributed various articles to reviews and other periodicals, of which I have not been able to obtain a complete list, but the following may be mentioned: A paper on Snowden—three papers on the Mosaic Cosmogony, in Tilloch's Journal—Papers on the Universities—on the Zodiac—on Isis and Osiris—on Faln and Schlegel—Articles on Delirium, Hypochondriasis, Somnambulism, Animal Magnetism, Soundness of Mind, and Temperament, in the "Cyclopædia of Practical Medicine;" and several chapters on similar subjects in the "Library of Medicine." Also a small volume on Insanity connected with Jurisprudence, and a highly interesting essay on the Vital Principle.

The study of the Hebrew language was alike congenial to his religious feelings and to his philological taste. An essay on the Song of Deborah, which he wrote for the gratification of his friends, is an interesting piece, in which, though short, the Doctor appears in both characters.

Study was so thoroughly identified with his life, that even the hours which he could spare from social intercourse were made subservient to his literary pursuits, and Greek readings with a few learned friends occupied the time which other men devote to light or frivolous pursuits. A poetical translation of the *Birds of Aristophanes* may be mentioned amongst the fruits of these *horæ subsecivæ*.

In the year 1826 the Doctor published the second edition of his "Researches into the Physical History of Man." In the interval of nearly thirteen years which had elapsed, he had not only collected a great amount of valuable materials, but had brought to bear upon the difficult questions which his subject presents a variety of collateral knowledge for their elucidation, thereby not only enhancing the value of his own researches, but pointing out to future inquirers the path to truth, in which he made such important advances. In the first volume he treated largely on the curious subject of the diffusion of organised beings, both vegetable and animal, entering into a most minute examination of a question which had previously occupied the attention of the great Linnæus, who maintained, that in every species of plants, as well as of animals, only one pair was originally produced. "Unum individuum ex hermaphroditis et unicum par reliquorum viventium fuisse primulùs creatum sana ratio videtur clarissimè ostendere."

In this edition increased precision was given to characteristic differences of form, complexion, hair and stature, the circumstances under which they occur, and the causes by which they may be influenced. The descriptions of the numerous families of mankind were greatly multiplied, and at the same time given with greater minuteness. But it must be observed, that in a work of this kind the author's own personal observations must, even in the case of a great traveller, be comparatively limited; whilst the author who writes in his own fixed residence, though he enjoys the largest amount of collected materials, must nevertheless be subjected to the serious inconvenience of being supplied with statements which may be either seriously defective, or absolutely inaccurate, without his being able at the time to correct or even to detect them. Renewed research and the division of labour are indispensable for the completion of the task, in the progress of which there will be much to interest and reward the ethnologist who will take Dr Prichard for his guide and instructor.

The diffusion of mankind presents one characteristic of the highest importance for its elucidation, which is altogether peculiar to our species. The characteristic to which I allude is that of *language*. It may be said, that in this respect it resembles many other characteristics resulting from the progressive cultivation of successive generations, which is the peculiar privilege of our race. Language, it is true, is subjected to the influence of this progressive cultivation, and preserves an important record of its advances. Yet there is, nevertheless, something peculiar in the subject of language, which places philology, as applied to the study of the human race generally, in a most exalted and important position amongst the abstruse sciences. I have only to appeal to the elaborate disquisitions of our learned associate, Dr Latham, for the proof of this assertion.

But to return to Dr Prichard. The philological portion of the subject, in the second edition of the work, was greatly enriched by a survey of the different relations of languages to each other; by the announcement of his discovery of the affinity of the Celtic languages with Sanskrit and other members of the Indo-European family; and by a tabular view of the known families of man, with their localities and languages, arranged according to their geographical distribution.

The affinities of the Celtic languages formed the subject of a separate volume, which Dr Prichard published in 1831.

To facilitate the appreciation of the value and importance, as well as of the difficulty of the discovery which it was the object of this work to exhibit, I may perhaps be allowed to offer a few brief remarks on the affinities of languages.

The degrees of affinity which may exist between languages are so very various, that it is absolutely necessary to define the meaning which it is intended to attach to the term *affinity*, as applied to languages. For want of a right understanding of this term, I have heard men, learned in many languages, seriously disagree as to the admission of such affinity. There are differences so slight as merely to affect the modification of words evidently the same. They scarcely affect the mutual intelligibility of the parties who use them. There is no dispute as to the identity of their language, and the differences are regarded as *dialectic;* but let parties meet each other with a somewhat greater difference of language, which prevents their interchange of ideas, and they will probably separate, each saying that the other speaks a different language. Such for example, might be the case were a Frenchman to meet with a Spaniard or an Italian, provided both parties were uneducated men. Yet the philologian, whether he regard the grammatical structure, or the derivation of the most ordinary words, would not hesitate to pronounce that the two languages are very closely related; and most readily to admit that they, and a few other European languages, such as the Portuguese and the Provençal, are twigs of the same bough. If one of the parties had happened to be a German or an Englishmen, there would have been the same mutual difficulty of comprehension; but the philologian would pronounce that the difference was more considerable; that instead of being twigs of the same bough, they might belong to boughs of the same branch. But besides discovering such a connection as would indicate this degree of community of origin, he would discover many words so far common to both, that they might be compared to the artificial union which the horticulturist may effect between branches towards their extremities after they had forked off below. It is in relation to the connection of languages, as branches proceeding from a common arm of the same tree, that modern philologians have made such great and important discoveries. Amongst the most remarkable of these discoveries is that of the affinity demonstrated by Jules Klaproth and some other German Philologists, between the Sanskrit and some other dead and living Asiatic languages and the Greek, Latin, German, and other languages, boughs of the same branch. The Celtic dialects, the remnants of the most ancient and westerly of the European languages, had not been shewn to belong to the same principal branch or arm; and I believe that it was doubted if such connection existed, until our late President, by means of his extensive acquaintance with numerous languages, and by a sagacious as well as perserving investigation of characteristics exhibited by the mode in which the changes of words and syllables are brought about, was enabled to make evident a connection dependent on community of origin, which must have existed at a most remote period, anterior to tradition as well as to history.

When we consider that there are languages so distinct that they cannot be brought within that very distant affinity which has been proved to exist between the Celtic and the Sanskrit, but which may be assembled together in one common group, like that which comprehends the American languages, amounting to some hundreds in number, and spoken from the North Frozen Ocean as far South as Terra del Fuego, by numerous tribes resembling each other in physiognomy more closely than the inhabitants of different districts of Great Britain, some idea may be formed of the interest as well as of the magnitude of the subjects which engage the attention of an Ethnologist who, like Dr Prichard, applied himself to the study of the human race as a whole.

If the accession of words received from a language of the same stock may be compared to the operations of horticulturists who unite the branches of the same tree, or if they more nearly resemble the anastomoses of bloodvessels, there are instances in which languages receive isolated words from languages of the most distant and distinct groups, which may be compared to the insertion of a graft from a totally different tree, or to the still more remote connection which exists between a parasitic plant and the tree to which it is attached. A familiar example of such introduction is furnished in our adoption of the word *taboo* from the South Sea Islanders. Now, it is possible for many such additions to be made, and indeed they have actually taken place in the opposite direction, the Polynesian language being enriched by European words, without any evidence being afforded of affinity between these remote languages. Such accessions, however, become important Ethnological characteristics, affording, it may be, the only records of the communications which have existed between distinct people. The history of the widely spread Polynesian race seems to admit of some such elucidation, from the traces which have been left by such introduction of Asiatic words.

It will be readily understood, that, by a man of Dr Prichard's learning and strong predilection for linguistic study, the philological element of Ethnology would be by no means underrated. In two able Reports, which he presented to the British Association for the Advancement of Science, he assigns to it its true and important place. In the Report of 1832, he successfully employed it as a corrective of classification founded on external characters only, which had led even the great and learned Cuvier to fall into palpable inaccuracies in his principal divisions of the human race.

In 1838, Dr Prichard published an Analysis of the Egyptian Mythology, which was a considerable extension of a former work which he had published on the same subject, with a critical examination of the remains of Egyptian Chronology. This earlier treatise had arrested the attention of German antiquarians, and the distinguished Professor A. W. von Schlegel had published a translation of it, with a preliminary essay. I am indebted to our associate, D. W. Nash, a common friend of Dr Prichard and myself, and who is also an Egyptian antiquarian, for the following notice of these works.

The discoveries of Dr Young, founded upon the inscription of the Rosetta stone, and the labours of De Sacy and Akerblad, had awakened great interest in Egyptian research in the minds of the learned of Europe. The great work of the French Scientific Commission, chief product of Napoleon's Egyptian expedition, had revealed the grandeur and extent of the remains of antiquity preserved in the valley of the Nile. The publication of M. Champollion's Letter to M. Dacier, in 1822, containing his hieroglyphic alphabet, gave promise that the obscurity which had so long enveloped the monuments of ancient Egypt would at length be dissipated. But, at the time when Dr Prichard published his "Analysis," the interpretation of the Egyptian historical monuments was a matter of hope and expectation only. It was not until the following year (1824), that Champollion's important work, the 'Précis du Système Hiéroglyphique des Anciens Egyptiens,' was presented to the public. The labours of Dr Prichard were therefore unassisted by and wholly independent of those monumental records which form the groundwork of recent Egyptian research.

But Dr Prichard was no mere Egyptologer. He took his stand upon a higher and broader ground, and treated the subject of Egyptian history as a branch of general ethnology,—a chapter in the great book of the Universal History of Mankind.

In his own words, in the preface to the first edition of his "Analysis," in 1823, the motive which originally induced him "to enter on the inquiries contained in this work, was the desire to elucidate, through the mythology of the ancient Egyptians, the relations of that people to other branches of the human family." It had frequently been asserted, and amongst others, by Champollion, that the Egyptians were a peculiarly African people, altogether distinct from the races of the Asiatic continent, and even wholly separate in origin from the rest of mankind.

It was particularly necessary for Dr Prichard to examine into the groundwork and foundation of such an opinion, so entirely at variance with the views deduced by him from his ethnological researches. The method which he pursued in the investigation, in this particular work, was a comparison of the mythological and philosophic doctrines and civil institutions of the ancient Egyptians with those which were developed among the worshippers of Brahma in Eastern Asia. The language of ancient Egypt was so entirely unknown, that no assistance could be derived from that source; the only method, therefore, which could be followed with any prospect of success, was the kind of analysis and comparison entered on by Dr Prichard. The result of this analysis undoubtedly presents a remarkable series of striking points of resemblance, in mythic dogmas, religious ceremonies, sacerdotal customs, cosmogonic and physical doctrines, and even, to a certain extent, in civil institutions.

This treatise was translated into the German language at the wish of Professor Welcke, of Bonn, and a preface to it written by the learned archæologist, Augustus William von Schlegel. Professor Schlegel, while paying a just tribute to the learning and acuteness of the author, and to the profound character of the work in question, combats the general conclusion derived by Dr Prichard from his comparison of Egypt with ancient India, in regard to the most important elements of their religion and political constitution. That general conclusion is, "that the same fundamental principles are to be traced as forming the groundwork of religious institutions, of philosophy, and of superstitious observances and ceremonies among the Egyptians and several Asiatic nations, more especially the Indians." It would be out of place here to enter at length into the character of the evidences adduced by Dr Prichard in support of this conclusion. The treatise itself presents an ample and methodical arrangement of the authorities on the subject of Egyptian mythology and philosophy, from the writings of Pagan and Christian authors. What remains of ancient literature and philosophy, bearing upon Egyptian history, has been copiously collected and carefully applied to the illustration of this obscure and intricate branch of the history of mankind. As in all other of Dr Prichard's writings, there is no straining of evidence to support a favourite hypothesis, but a careful statement of facts and circumstances, with a view to the elucidation of truth. The conclusion drawn from the remarkable coincidences and relations which Dr Prichard pointed out as existing between Egyptian and Indian modes of thought, has received considerable support from a quarter the least expected. Recent investigations into the structure of the old Egyptian language, revealed to us by the successful interpretation of the hierogrammatic writing, have demonstrated an early original connection between the language of Egypt and the old Asiatic tongues. By this discovery, the Semitic barrier interposed between the Egyptian and the Asiatic races is broken down, and a community of origin established, which requires the hypothesis neither of the immigration of sacerdotal colonies, nor the doubtful navigation of the Erythræan sea. The profound views which led Dr Prichard to assert, that, "although many obstacles present themselves to the supposition that direct intercourse subsisted between the Egyptians and the nations of Eastern Asia, there appear, even on very superficial comparison, so many phenomena of striking congruity in the intellectual and moral habits, and in the peculiar character of mental culture displayed by those nations, and particularly by the Egyptians, when compared with the ancient Indians, that it is extremely difficult to refer all these analogies to merely accidental coincidence," have thus been remarkably confirmed. His comparisons of individual personages of the mythologic system of either nation may not bear the test of measurement by the more extended knowledge of the subject which a quarter of a century has produced; but the terms of the general conclusions which are deduced from his "Analysis" may be fairly taken to be past all dispute.

The "Critical Examination of the remains of Egyptian Chronology" is a remarkable monument of Dr Prichard's sagacity, and of his aptitude for the elucidation of an obscure and intricate subject. The difficulty of the task which he here undertook he has not overrated, when, after laying before the reader the lists of Manetho and Eratosthenes, the old Chronicle, and the dynastic chronology of Herodotus and Diodorus, he says, "nothing can be more discouraging than the first survey of the fragments we have extracted. When I first examined these fragments, with a view of computing from them the Egyptian chronology, they appeared to me to be an inextricable tissue of error and contradiction. I repeated my attempt several times, at intervals, before I obtained the smallest hope of success, or a ray of light to guide me through the labyrinth. At length I thought I discovered a clue, which I have followed, and have persuaded myself that it has enabled me to unravel the mystery."

That clue was discovered by the same kind of investigatory process which has been applied in all Dr Prichard's researches,—the obtaining fixed points of coincidence or agreement, with which to form a standard of comparison for apparently discordant materials.

Discordant as the several lists of the Egyptian Pharaohs appeared, there were various points of agreement and correspondence between them, clearly demonstrating a derivation from some common source. The collation of the various lists, thus shewn to possess a certain authenticity, produced a series of historical synchronisms, which served as fixed points for computation in an upward and downward direction.

Rejecting the untenable doctrines of Marsham and Scaliger as to the contemporaneous character of the several dynasties of Manetho, and the division of Egypt into various districts and independent kingdoms, whose sovereigns appear in the lists in a false order of succession, Dr Prichard commenced by treating the various historical fragments as authentic history, whose discrepancies were capable of being reconciled by the application of judicious critical comparison. Professor Schlegel imputes to him, as a fault inherent in an English author, a want of frankness and of freedom from prejudice, which causes him to incline, in his chronological views, "to the errors of the Harmonists, who, for the last 1500 years, have been vainly labouring to bring into seeming accordance the contradictions of the so-called profane history and of the traditons which are deemed sacred." How little this reproach, if it be one, was deserved, is evident, not only from the general tenor of the investigation pursued, but from the author's own statement of the rule by which he was guided in his research. "Various attempts," says Dr Prichard (*Critical Exam.*, p. 88), "have been made to reconcile the chronology of Manetho with that of Moses. Perizonius allows the Egyptian annalist to be correct through the latter half of the chronicle; but not knowing what to do with the first fifteen dynasties, he boldly erases them at once, and declares them to be a forgery of the author. He has been followed by several later authors, particularly by Dr Hales. This way of proceeding is more like cutting the Gordian knot than untying it. We have no right to act in so summary a manner. If we cannot reconcile the antiquity assumed by the annals of one nation with the dates assigned for the origin of empires and of the world in the records of the others, we have no other course to pursue than to acknowledge the contradiction between them. We may have good reasons for placing confidence in one record rather than another; but we have no right to cut off from the archives of Egypt all that extends too far, as if we were shortening the limbs of Procrustes, and then pretend that we have reconciled them with the computation of the Hebrew Scriptures.

"But though we ought to abstain from new modelling the Egyptian antiquities after the pattern of the Hebrew, no objection can be made to our comparing all the documents we possess that relate to the chronology of Egypt, and endeavouring to find some method of reconciling them with themselves. We are only bound, while proceeding in this attempt, to exclude all prejudice in favour of those particular methods that lead to conclusions which we are, from other considerations, inclined to adopt."

These are undoubtedly the sentiments of genuine historical criticism.

The view taken by Dr Prichard, founded on the internal evidences of the documents themselves, as to the relative characters of the lists of Manetho and Eratosthenes, is in its leading features, and especially as relates to the earlier period of the Egyptian chronology, fully borne out and confirmed by later experience.

The conclusion deduced from a comparison of the lists that the third, fourth, and sixth of Manetho contain a succession coeval with that of the first twenty-two sovereigns of the Latenculus of Eratosthenes, is very nearly the same with that arrived at by the Chevalier Bunsen, aided by an examination of original and all but complete monumental and documentary chronological records of Egypt. Bunsen makes the first twenty-two sovereigns of Eratosthenes correspond to the first, third, fourth and sixth dynasties of Manetho, rejecting from the list of Manetho the second and fifth dynasties, as had been done by Dr Prichard.

That in other points the chronological comparisons instituted by Dr Prichard should not have been confirmed by subsequent disoveries, is by no means extraordinary. Unaided by the evidence derived from the monuments, the analysis of Egyptian chronology, immediately subsequent to the Hyksos domination, is far more difficult and more intricate than for the preceding period. To the conquering monarchs of the eighteenth and nineteenth dynasties are ascribed the myths and traditions which belong of right to the heroes of a remoter age ; and an investigation, based of necessity solely on a comparison of names and fragmentary historical notices of individual sovereigns, is involved in an endless maze of conflicting testimony. Professor Schlegel has truly observed of this treatise, that the learned industry and the intelligence of the procedure of its author are worthy of all commendation ; and it may be safely affirmed, that its production at a period when the chronology of Egypt was almost a blank in history, is an enduring testimony to the critical acumen and profound sagacity, no less than to the extensive learning, of its author.

Dr Prichard's singularly retiring manners kept him much aloof from public affairs ; yet, when occasion required it, he could exert himself with successful zeal. He felt personally interested in the importance of placing the means of a liberal education within the ready access of the youth of Bristol ; and with the co-operation of several gentlemen in his neighbourhood, amongst whom may be mentioned his particular friends Eden, Tothill, and Conybeare, he established the Bristol College, and he had the satisfaction of seeing one of his own sons amongst the first who acquired distinction under its professors.

Dr Prichard's interest in the varieties of the human race was not limited to making their physcial characters, their languages, their manners, and often obscure history, the objects of scientific or learned research. He felt the interest of a philanthropist and a Christian, in the protection and amelioration of the weak and oppressed branches of the human family. He hailed the formation of the Aborigines' Protection Society, and was one of its early advocates. Though his residence at Bristol did not allow him to take an active part in the Society, his name was on the first list of its honorary members; and I may be allowed to quote the following passage from his pen, which was printed in one of the earliest of the Society's publications:—

"I much regret that circumstances over which I have no control will prevent me from attending the Anniversary Meeting of the Society for the Protection of the Aborigines. I hardly need say to you that there is no undertaking of this comparatively enlightend, and, as I trust it may be called, Christian age, which appears to me calculated to excite a deeper and more lively interest than this truly admirable attempt to preserve from utter ruin and extermination many whole tribes and families of men, who, without such interference, are doomed to be swept away from the face of the earth. Certainly there is no undertaking of the present time that has a stronger claim on humanity, and even on the justice of enlightened men. For what a stigma will be placed on Christian and civilized nations when it shall appear, that, by a selfish pursuit of their own advantage, they have destroyed and rooted out so many families and nations of their fellow-creatures, and this, if not by actually murdering them, —which indeed appears to be even now a practice very frequently pursued,—by depriving them of the means of subsistence, and by tempting them to poison and ruin themselves. For such a work, when it shall have been accomplished, the only excuse or extenuation will be, just what the first murderer made for the slaughter of his brother; and we might almost be tempted to suppose that the narrative was designed to be typical of the time when Christianized Europeans shall have left on the earth no living relic of the numerous races who now inhabit distant regions, but who will soon find their allotted doom, if we proceed on the method of conduct thus far pursued, from the time of Pizarro and Cortez to that of our English Colonists of South Africa. But independently of the claim of humanity and justice which this admirable undertaking presents, there are numerous points of view in which it is particularly interesting to the philosopher and to men devoted to the pursuit of science. How many problems of the most curious and interesting kind will have been left unsolved if the various races of mankind become diminished in number, and when the diversified tribes of America, Australia, and many parts of Asia, shall have ceased to exist! At present we are but very imperfectly acquainted with the physiological character of many of these races, and the opportunity of obtaining a more accurate and satisfactory knowledge will have been for ever taken away. The physical history of mankind, certainly a most interesting branch of human knowledge, will have been left for ever imperfect, and but half explored,"

I know that Dr Prichard had the Aborigines' Protection Society in view in giving an important paper on the Extinction of Races, to the British Association for the Advancement of Science at its Meeting in Birmingham in 1838.

On accepting the office of Inspector of the Lunatic Asylums, Dr Prichard relinquished private practice, resigned his post as Physician to the Infirmary, which he had held for more than twenty-six years, and transferred his residence from Bristol to London. To this change our Society is indebted for the privilege which we have enjoyed of having the greatest of ethnologists as our President. He succeeded our first President, Sir Charles Malcolm, to whose able exertions at its origin, and during the progress of its formation, the Ethnological Society of London is incalculably indebted.

After his settlement in London, Dr Prichard completed the third edition of his work, which extended to five closely-printed volumes, forming a mass of learned and scientific research and laborious compilation far superior to anything which had been previously produced on Ethnology, and scarcely surpassed in the literature of any other science.

In this Edition Dr Prichard introduced the distinctive appellations of Stenobregmate and Platybregmate, as characteristics of different forms of skull; and he subsequently gave directions for the different aspects in which skulls are to be viewed for the purpose of noticing ethnological points. A somewhat analogous service has been performed by the distinguished Professor Retzius of Stockholm, who, having devoted special attention to this part of Ethnology, has classified nations according to the prevalent forms of their heads, and employed the distinctive terms, Dolico-cephalic and Brachycephalic, each of which are again divided into Prognate and Orthognate.

Having myself paid some attention to the ethnological grouping of human skulls, I must confess that I have found very considerable difficulty in adopting points of characteristic difference; and in this very difficulty I find an argument in favour of the unity of our species, and of the differences which we observe being those of variety only. I cannot adduce a better illustration of this remark than that which is afforded by the skulls and portraits of American Indians. The unmixed Indians of North and South America form as well marked and distinct a group of the human race as can be pointed out; and I have noticed greater differences in the form of the head between individuals of the same tribe, than between those of individuals of different tribes, separated from each other by thousands of miles, and between which the most remote connection cannot be traced.

Having already noticed the principal divisions of the subject in speaking of the Doctor's previous writings, I will not now trespass on the time of the Society with any further observations on this third edition. Whilst the publication of this great work was in progress, Dr Prichard produced a smaller one on the same subject, which appeared in illustrated numbers, designed to encourage and popularize the study of ethnology by consulting the taste of the day. On the completion of the larger work, Dr Prichard observed that he considered his literary labours as accomplished; yet we cannot doubt, that, had his life and health been spared, his ever active mind and confirmed habits of study and labour would have continued to gratify and instruct us by further productions of his well-stored mind; in fact the subject of my last conversation with him, as we walked together from the last meeting of this Society at which he presided, was the publication of a collection of plates of human skulls, illustrative of ethnology, somewhat on the plan of the "Crania Americana" of my friend Dr Morton, of Philadelphia.

It cannot fail to be a matter of surprise and wonder, when the nature of the Doctor's private practice, and the character of his official duties, which called him much from home, are considered, how he was able to accomplish so much. I have been informed that he not only had acquired the rare and invaluable habit of saving and occupying those detached fragments of time which it is most difficult not to lose, but that he also possessed the remarkable faculty of being able at once to resume and proceed with his compositions at the point at which he had left them.

Dr Prichard appeared to be in possession of his usual health till within a few weeks of his death; yet it is probable that the unusual dampness of the latter part of the last year, to which may be ascribed the remarkably low and atonic characters of almost every case of illness, had produced a latent influence on his system, and prepared it to yield to the exciting causes which were applied.

He had left his home, and was engaged in one of his official tours, when he was seized with a severe feverish attack while visiting the Lunatic Asylums in the neighbourhood of Salisbury, on the 4th of December 1848, and was confined in that city until the 17th, when he was conveyed to his own house in London. The fever proved to be of a rheumatic and gouty character, baffling all the efforts of medical skill, and terminating his life, after much suffering, by pericarditis (inflammation of the membrane containing the heart) and extensive suppuration in the knee-joint.

As a practitioner of medicine, Dr Prichard was remarkable for decision on the character of disease, and for a promptness and energy in the application of remedies. Many have been the instances where, in extreme cases, the boldness of his practice was followed by unexpectedly happy results. In his intercourse with professional brethren and colleagues his conduct was straightforward, honourable, and generous: to his patients he was gentle, attentive, and kind.

High moral and religious principle, an affectionate disposition, an instinctive sentiment of delicacy, propriety, and consideration of the feelings of others, and retiring modesty and simplicity of deportment, as much distinguished and endeared him in the domestic and social relations of life, as his literary and scientific attainments elevated him to the eminence he held in public estimation; he furnished, indeed, a bright example of the scholar, the gentleman, and the Christian.

Dr Prichard's great attainments and learned and important works justly acquired universal reputation, and the honours and distinctions of Literary and Scientific Societies were poured in upon him. When he attended the meeting of the Provincial Medical Association at Oxford, the University conferred upon him the Doctor's degree. The National Institute of France elected him a Corresponding Member,* and he received the same distinction from the Academy of Medicine and Statistical Society there, from the Academy of Natural Sciences of Philadelphia, the American Philosophical Society, the Oriental Society of America, the Ethnologial Society of New York, the Scientific Academy of Vienna, and from other bodies. He was likewise Fellow of the Royal Society, and Member of the Royal Irish Academy, and of the Royal Geographical Society.

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* I cannot deny myself the pleasure of stating a fact in relation to the Doctor's election to the distinguished honour of Corresponding Member of the Institute of France. Whilst paying a visit to Paris, in conversing with one of my friends who was a member of the Institute, he talked of nominating some English associate, and proposed one or two names, which led me to suggest that of Dr Prichard. It was highly approved by my friend, who consequently brought it before his colleagues, and the Doctor was elected accordingly.

A Description of several extraordinary Displays of the Aurora Borealis, as observed at Prestwich, during the winter of 1848–1849; *with Theoretical Remarks.* By WILLIAM STURGEON, Lecturer on Natural and Experimental Philosophy, formerly Lecturer at the Honourable East India Company's Military Academy, Addiscombe, and late Editor of the "Annals of Electricity," &c. Communicated by the Author.

(Concluded from page 158.)

Theoretical Views.

With respect to the cause of this meteor, I can form no other opinion, than that it originates in a sudden change of temperature in the upper regions of the atmosphere, which gives rise to a corresponding disturbance of the electric fluid, causing extensive movements of it amongst the attenuated air and aqueous vapour, illuminating them as it spreads in various directions, according as their different parts are prepared for its reception and diffusion. It is no unusual circumstance to observe preparations as it were, during the evening, before daylight has disappeared, for a display of auroral beams or streamers after nightfall. These preludes consist of certain arrangements of thin streaks of nubiferous matter, floating at high altitudes, and often stretching quite across the heavens, and appearing to converge at two opposite points near the horizon; forming what some people call *Noah's Ark.* These streaks or bands of vapour, when traversed by the electric fluid at the night time, become *luminous conductors,* and form streamers of the aurora borealis; displaying different degrees of brilliancy, in correspondence with the attenuation of the nubiferous arrangement and the quantity of electric fluid flowing through it. From this simple fact, which I have myself witnessed, and from the high probability that similar arrangements of still more attenuated aqueous vapour are frequently formed at altitudes where they are far beyond the reach of observation, until illuminated by electrical disturbances, there can appear no great degree of extravagance by supposing that most, if not all, streamers assume their peculiar forms from a like cause. It is possible, however, that on many occasions, the electrical disturbances may take place even at higher altitudes, and the light be transmitted through the thinnest bands of these nubiferous arrangements, which would give the appearance of streamers or luminous beams, as decidedly as if they were themselves the conductors or channels of electrical transmission.

The streamers, which mostly constitute a conspicuous feature in the aurora borealis, are not often suddenly formed; they generally spring from some definite speck in the heavens, and wax gradually to their full dimensions, and then as gradually fade away. Some streamers, it is true, shoot rapidly to their full growth, and almost as suddenly disappear; but in all cases, they can be seen expanding lengthwise, whatever may be the rapidity of their growth; they sometimes lengthen in both directions, but most frequently in one direction only, a circumstance more favourable to the idea of the nubiferous bands being the media of transit, than in the capacity of transparent screens, permeated by an electric light from above.

In many displays of the aurora, floods of streamers *appear* to flow upwards, from every part of a luminous bow, which crosses the meridian in the north, and stretch to various angles of altitude towards the spectator; some of them reaching to his zenith, whilst others terminate their career before they arrive midway, but in no instance do streamers spring into existence mature or full grown. This fact also gives countenance to the idea of their consisting of streaks or bands of thin aqueous vapour, gradually, though rapidly in some cases, illuminated longitudinally, by transmissions of the electric fluid. This view is still further supported by the fact that, in whatever direction streamers may be elongated, the point from which they spring is the most intensely luminous of the whole, and becomes the *base* of the group; from this base or starting point, the light becomes more and more attenuated, until at last it softens gradually and melts into the normal light of the sky and is lost. This gradual decay of brilliancy during the progress of streamers, from their birthplace to their terminal points, has every appearance of a gradual dispersion, and consequent attenuation of the electric fluid, as it flows along the aqueous conductors, until eventually it becomes so enfeebled as to be incapable of displaying a sufficiency of light to be traced by the eye, any further in its progress towards its destination. Hence also, the different distances to which streamers reach from their respective birth-places; some fade away and are lost within a range of a few degrees, whilst others progress through an immense span in the heavens, but in all cases terminating in nearly the same manner.

Nor are these the only indications of the aurora being within the limits of the atmosphere. The colour of the meteor is not that of an electrical light in a vacuum, nor in very highly attenuated air; but such as would be produced by floods of the electric fluid amongst attenuated aqueous vapour. The auroral light is that of a pure candle flame, and sometimes of a silvery white, neither of which can be imitated by electrical transmissions through a vacuum.

Philosophers have long been endeavouring to ascertain the height of the auroral arch, when displayed in the northern heavens, but hitherto, no two of them have arrived at similar conclusions. Some have supposed it to be only a few miles above the earth's surface, and others have given it a height of above a thousand miles. From some calculations made by the late Dr Dalton, he infers that "the height of the *rainbow-like arches* of the *aurora*, above the earth's surface is about 150 English miles."—(*Meteorological Observations*, &c.)

If the aurora be an electrical meteor, as is now generally admitted, the northern arch, which invariably assumes the white flame colour, must necessarily be situated within the limits of the atmosphere, for the reasons already stated; the streamers also, which are generally of the colour of the arch, are obviously within a similar range from the earth's surface, and from the hazy condition of the air through which they sometimes appear to progress, added to the fact, that they often conform themselves to the figure and position of certain formations of cloud, there appears much reason to believe, that streamers are displayed within the regions of aqueous vapour.

With respect to the apparent ascent of streamers, it is nothing more than the effect of perspective, and ought not to be understood, that those parts of the meteor rise to a higher region from the earth's surface; but, in the same sense as a cloud is said to rise, or the sun, moon, or any other heavenly body is understood to rise, which implies no increase of distance between the earth and the body, but merely an increase in the vertical angle, formed by visual ray from the body, and the plane of the horizon; it is therefore the elongation of a streamer *towards* the zenith of the observer, that causes the appearance of ascent or shooting upwards, and no real increase of distance from the earth's surface. When streamers pass the zenith, their increase in length gives them the appearance of a *downward* motion, which is also the case, whatever may be their course, provided the progress of elongation is *from* the spectator.

From the observed influence of electrical forces in giving forms to and producing intestine commotions in thunder-clouds, there is reason to infer, that similar forces are productive of peculiar forms and arrangements of aqueous vapour, in regions much higher than those groups of heavy clouds; and that electrical transmissions may be accomplished through conductors which had been formed by electrical forces, but such formations of conducting material could take place exterior to the atmosphere, where none is in existence.

It is a well-ascertained fact, that the electric fluid is more abundant in the upper parts of the atmosphere than in the inferior strata; and that this is the normal state of the air when undisturbed by clouds or other causes. Hence, were this normal state to remain unruffled, there would be a steady equilibrium of electric forces throughout the atmosphere, and an electrical tranquillity would be permanently established in every part of it. Such a tranquillity, however, cannot possibly exist in an atmosphere that is subject to continual fluctuations of temperature, moisture, and consequent winds; hence the natural tendency to an electrical equilibrium is ever being interrupted, and electrical commotions, of more or less magnitude, are continually going on.

When the air is highly charged with aqueous vapour, and suffers a sudden depression of temperature, dense clouds are formed with amazing rapidity, and the electric fluid being condensed in them to a higher degree of intensity than they can retain it, liberates itself from this aqueous imprisonment in the shape of lightning. In the upper regions of the air, where the insulation is much less perfect, no lightning cloud can possibly be formed; because the electric fluid finding but little resistance to its movements, however suddenly it may be distubed, by change of temperature, flows from one part to another before its intensity gets sufficiently high to form lightning or discharge itself in a close compact body; hence any sudden disturbance of the electric fluid in the upper regions of the atmosphere, instead of producing lightning, would cause it to move in waves, or as a diffused ocean, in those directions offering the least resistance; covering an extensive area in its transit; such an electric tide, occurring at night-time, would be visible, and partake of all forms of the conducting media through which it passed.

Now, it being a well-established fact that attenuated air is a better conductor than air of greater density, and that a vacuum is a better conductor then attenuated air; and as the attenuated regions of the atmosphere are more highly charged with the electric fluid than the dense air below; analogy would lead to the inference, that the electric fluid is still more abundant exterior to the shell of air than anywhere within it; an inference which will readily be conceded by those who allow that the aurora borealis is an electrical phenomenon displayed at elevations far beyond the reach of the atmosphere. But here it is that an insuperable difficulty presents itself in finding the disturbing agent. Within the atmosphere, electrical disturbances are easily accounted for by the influence of well-known agents; but at the distance that some philosophers have placed the aurora from the earth, such agents are not known to exist.

The electrical theory of the aurora borealis, as it exists at the present day, is considerably alloyed with the magnetism of the earth. Halley appears to be the first on the list of those philosophers who have called in terrestrial magnetism to assist in explaining the cause of the aurora borealis; but the circulating *magnetic effluvia* of this eminent philosopher appearing insufficient for the views of Dalton, the latter invented "an elastic fluid partaking of the properties of *iron*, or rather of *magnetic steel*," which he placed in the upper regions of the atmosphere, in "the form of cylindrical beams," which, when illuminated by the electric fluid, become the beams or streamers of the aurora borealis; and "the rainbow-like arches," says this philosopher, "are a sort of rings of the same fluid, which encompass the earth's northern magnetic pole, like as the parallels of latitude do the other poles.*

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* Meteorological Essays, p. 169.

The frequent position of the auroral arch with respect to the magnetic meridian, and the occasional disturbance of the magnetic needle during an auroral display, are well calculated to associate terrestrial magnetism with the theory of the meteor; but it would be difficult to imagine how the extravagant appendages of Dalton could be required to give " an irregular oscillation to the horizontal needle," which amounted to no more than " half a degree" on each side of its " mean daily position;"* especially as the principles of electro-magnetism were as well known at the time the last edition of the Hypothesis was published (1836), as they are at the present day; and would as easily have accounted for the needle's movements independently of those appendages as with them; and although other observers have met with much greater movements of the magnetic needle during an aurora, I can see no reason for supposing that they were due to ferruginous matter, floating in the atmosphere, because the well-known principles of electro-magnetism are, independently of any such ferruginous elements, quite sufficient to accomplish their production.

The hypothesis of Dalton, with some slight modifications, being that in most repute at the present day, and favoured by the views of some of our most illustrious philosophers, require more than an ordinary consideration; and, fortunately, being expressed in terms that cannot well be misunderstood, it may be examined without any apprehensions of mistaking the principles on which it is founded.

In order that our author might not be obscure in his views, he particularly states the difference between the *magnetic effluvia* of Halley, and the *ferruginous matter* of which he constructs his cylindrical magnetic beams. " It may perhaps be necessary here, before the subject is dismissed," says Dalton, " to caution my readers not to form an idea, that the *elastic fluid of magnetic matter*, which I have all along conceived to exist in the higher regions of the atmosphere, is the same thing as the *magnetic fluid or effluvia* of most writers on the subject of magnetism. This last they consider as the efficient cause of all the magnetic phenomena; but it is a mere hypothesis, and the existence of the *effluvia* has never been proved. My *fluid of magnetic matter* is, like magnetic steel, a substance possessed of the properties of magnetism, or, if these writers please, a substance capable of being acted upon by the magnetic *effluvia*, and not the magnetic *effluvia* themselves."

It is somewhat remarkable that, after such an abrupt dismissal of all preceding attempts at explanation, the hypothesis of Dalton should appear the most extravagant that has hitherto appeared in the history of the aurora borealis. We have no knowledge whatever of the existence of this imaginary *ferruginous effluvium;* nor would any magnetist ever suppose that such a fluid, even were it admitted to have an existence, would put on the form of *cylindrical beams*, and at the same time adapt itself into *rings* round the magnetic poles of the earth. Moreover, as this imaginary fluid is supposed to be floating within the atmosphere, the hypothesis is left in a state of imperfection from a want of information respecting the author's mode of expanding the shell of air to the thickness of 150 miles, the height at which he has placed the aurora borealis.

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* Meteorological Essays, p. 171.

The same hypothesis supposes that the auroral beams "are similar and equal in their real dimensions to one another," an assertion by no means sanctioned by observation; but, on the contrary, perfectly at variance with the appearances generally. The hypothesis also supposes that the auroral beams are all "parallel to the *dipping needle* at the places over which they appear;" and that "the point in the heavens to which the *beams* of the *aurora* appear to converge at any place, is the same as that to which the south pole of the *dipping needle* points at that place." With all due respect for the philosophical ability and skill of Dr Dalton, the cause of science has a predominating claim to our regards over all other considerations in discussions of this nature; there can, therefore, be no impropriety in stating that, were there no other observations to discountenance this part of the hypothesis, those on the aurora of the 17th November last would be sufficient to prove its inaccuracy.

That the auroral arches, when they appear in the north of these latitudes, cross the magnetic meridian at nearly right angles, is a fact very frequently observed, though it is by no means its universal position. The highest point of the arch is probably as frequently in other positions as in the magnetic north: it is sometimes several degrees eastward of the true north, at other times due north; and, on many occasions, it never appears at all. To admit that the arch is a visible part of a complete ring that surrounds the magnetic pole of the earth, and that at the same time it crosses, at right angles, the magnetic meridians of every place of observation, would be to admit a complete system of confusion—in fact, an absurdity. According to Hansteen's and Barlow's maps, the curve of *equal variation* that passes through Great Britain, passes also a little north of the Western Isles, through Newfoundland and into Hudson's Bay; and in the other direction, it passes through the North Sea, the Shetland Islands, and thence almost direct north past Spitzbergen. No *circular ring* that could possibly be imagined to surround the north-western magnetic pole of the earth, would answer the other parts of the hypothesis for all the magnetic meridians of that particular *curve of equal variation*. In Hudson's Bay, the magnetic meridian would be at right angles to the magnetic meridan of Great Britain; and in many parts of the curve there would be such obliquities of the magnetic meridians to each other, that but very few of them would cross tangents to the supposed ring, at right angles, and at the point of contact,—circumstances required by the hypothesis; or, in other words, there are but very few magnetic meridians in this *curve of equal variation,* that run sufficiently close to the north-western magnetic pole of the earth, to satisfy the conditions of the hypothesis. In Great Britain, and throughout the northern parts of the curve, the magnetic meridians pass *north* of the pole; and the magnetic meridian, for the same curve, opposite the coast of Spitzbergen, would pass the magnetic pole northward upwards of 10° of latitude. Westward, over the Atlantic Ocean, the magnetic meridians of this curve would approach the north magnetic pole more closely; and in Newfoundland, the magnetic meridians would probably pass through that pole; but on the American Continent, towards Hudson's Bay, the magnetic meridians would pass the north-western pole, some 3° or 4° on its south side.

I have selected this particular curve of equal variation, as it stands in Professor Barlow's map, because it is that which passes through the north of England, and corresponds with the variation at Kendal (25°), when Dr Dalton made his observations. Had the selection been made on the curve of 20°, which passes through the most western parts of Spitzbergen, through Norway, France, Algiers, and the Canary Islands, thence across the Atlantic to Nova Scotia, Canada, and Hudson's Bay, the deviations of the magnetic meridians from the magnetic pole would have been much greater, especially in Europe, where the meridians have been more exactly ascertained than in any other part of the curve,—that is, through the whole of the curve from the Mediterranean to Spitzbergen; in which the magnetic meridians would cross the meridian in which the north-western magnetic pole is situated on its northern side, and the magnetic meridian of Spitzbergen would cross the meridian of the pole 15° or more north of it. In the western portion of the curve, however, from Algiers to Hudson's Bay, the magnetic meridians would run sufficiently close upon the magnetic pole to answer the conditions of the hypothesis. But the deviation of the *western line of no variation,* from the meridian of the magnetic pole, would alone be sufficient evidence of the incorrectness of the hypothesis.

In all these cases, the north-western magnetic pole is supposed to be situated in 69° 53′ north latitude, and 93° 33′ west longitude, as calculated for the year 1800, which is the nearest date on record to 1793, the year in which Dr Dalton first published his theoretical views of the aurora borealis, and which are those that still appear in his last edition, published in 1836.

In referring again to that part of the hypothesis which places the auroral beams "parallel to each other," and at the same time, "parallel to the *dipping needle* at the places over which they appear," it is obvious, that, to fulfil these conditions, the dipping needle would have to assume one and the same position, both in dip and direction, at all places *over which the* auroral beams appear at any one time;

that is, the dipping needle would have to be parallel to one individual right line, at all places of observation, however wide apart. Now, without taking into consideration the difference of dip at different places over which auroral beams often appear at the same time, the different positions of the magnetic meridians, in the planes of which the axis of the dipping needle would repose at those places, would be quite sufficient to shew the fallacy of that part of the hypothesis.

It is somewhat remarkable, that neither Dalton nor any other philosopher that I am aware of, has taken into consideration the electro-magnetic forces of auroral beams or streamers, in disturbing the magnetic needle. These forces are brought into play by every movement of the electric fluid, whether ferruginous or other metallic matter be present or not,—as well in the most perfect vacuum as in dense air; and when such immense floods of the electric fluid are put into motion as constitute a grand aurora borealis, the principal features of which are extensive groups of streamers, it is to be expected that the electro-magnetic forces of those streamers will disturb the compass-needle, causing deflections of different degrees of magnitude, and in different directions, in correspondence with the intensity and direction of the disturbing forces; and, all other things being the same, the greatest deflections of the horizontal needle would be accomplished by electric streamers that were parallel to it, and, consequently, parallel to the earth's surface, at the place of observation. The supposition of the auroral beams being vertical, or nearly so, and at remote regions above the atmosphere, may possibly have been the cause of the electro-magnetic forces of streamers being so generally overlooked; but it has long appeared to me that, to their influence the observed disturbances of the needle are principally if not solely owing.

Although the theoretical views which I have taken dispenses entirely with the ferruginous effluvium supposed to be floating in the air, I by no means attempt to deny its absolute existence, nor the existence of other metallic effluvia. My motive for contending against the influence of such an agent in producing auroral beams, is to shew that it is quite unnecessary for the purpose it was intended, and the manner in which it has been applied preposterous. The manner in which I have attempted to explain the several phenomena attending the aurora borealis, requires no other elements nor forces than those well known and understood. The same cause, a sudden depression of temperature, that produces lightning amongst the clouds, would produce an aurora borealis in a higher region of the air; a depression of temperature at the earth's surface invariably succeeds a lightning-storm, and almost as certainly closely follows an aurora borealis; and very often both of these electrical phenomena appear at the same time,—shewing that the disturbance extends to a great height in the atmosphere, and the fall of temperature below, that succeeds these phenomena, as well as the immense hail-storms that attend lightning, infer that the change of temperature commences far above the earth's surface, and that it progresses *downwards* with various degrees of speed.

The observations which I have myself made on the predisposition of clouds for a display of the aurora borealis, are similar to the observations of Captain Back at Fort Reliance, (north latitude 62° 46′, and west longitude 109°.) "The aurora was frequently seen at twilight, and as often to the eastward as the westward. Clouds, also, were often perceived in the daytime, in form and disposition very much resembling the aurora."

The same scientific officer observed also, that "a dense fog, in conjunction with an active aurora, was uniformly favourable to the disturbance of the needle;" and, "when seen through a hazy atmosphere, and exhibiting the prismatic colours, almost invariably affected the needle." These observations are of great interest, both as regards the decomposition of the electrical light, and the production of magnetic disturbance: shewing, in my opinion, that the fog was essential in the production of colours, as well as to the transit of a portion of the electrical fluid at no great distance from the needle; and this view is strongly supported by the opposite effects of the aurora when no fog or haze was present. "On the contrary," says Captain Back, "a very bright aurora, though attended by motion, and even tinged with a dullish red-yellow, in a clear blue sky, seldom produced any sensible change (of the needle) beyond, at most, a tremulous motion."*

There is a great difference in the character of auroral displays, scarcely any two being alike: some of them appear to be of such a complex and mysterious character as to bid defiance to scientific investigation: whilst others develop a peculiarity of features that can hardly be misunderstood; and may, with propriety, be considered as keys of admission to the whole. Amongst the latter may be enumerated those in which are observed a predisposition of nubiferous matter during daylight,—the luminous streaks or bands of vapour,—the waves of light that shine across the eye of the spectator,—the hazy character of the atmosphere,—and also the transcolourations of the light; all of which have appeared in unusual abundance during the past season, or since the commencement of last autumn.

Dr Halley gives a very precise account of the appearance of luminous vapour, haze, and streaks of light, in the aurora of 16th March 1716. This eminent philosopher tells us, that he did not see the aurora till about nine o'clock; but, at that time, he "immediately perceived, towards the south and south-west quarter, that, though the sky was clear, yet it was tinged with a strange sort of light; so that the smaller stars were scarce to be seen, and much as it is when the moon of four days old appears after twilight. I perceived, at the same time, a very thin vapour to pass before us, which arose from the precise east of the horizon, ascending obliquely, so as to leave the zenith about fifteen or twenty degrees to the northward. But the swiftness wherewith it proceeded was scarce to be believed, seeming not inferior to that of lightning, and exhibiting, as it passed on, a sort of momentaneous *nubecula*, which discovered itself by a diluted and faint whiteness; and was no sooner formed, but before the eye could well take it, it was gone, and left no signs behind it. Nor was this a single appearance; but for several minutes, about six or seven times in a minute, the same was again and again repeated, these *waves of vapour*, regularly succeeding one another, and at intervals nearly equal, all of them in their ascent producing a like transient *nubecula*. By this particular we were at first assured, that the *vapour* we saw became conspicuous by its own proper light."

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* Mr Dancer, optician, of Manchester, whilst observing the white light of one of the auroræ described in this paper, breathed upon a pane of glass through which he was looking, and immediately the prismatic colours appeared. The same effect is produced to passengers travelling in a close coach, and looking at the gas-lights through a window covered with aqueous particles from breathing; *a beautiful prismatic iris* is seen around the burning gas.

In this noted aurora, there was no light seen in the north till about eleven o'clock. "On the western side of the northern horizon, viz., between west and north-west, not much past ten o'clock, I observed," says our author, "the representation of a very bright twilight, contiguous to the horizon, out of which arose very long beams of light, not exactly erect towards the *vertex*, but something declining towards the south,—which ascending by a quick and undulating motion to a considerable height, vanished in a little time, whilst others, at certain intervals, supplied their place. But, at the same time, through all the rest of the northern horizon, viz., from the north-west to the true east, there did not appear any sign of light to arise from, or join to, the horizon, but what appeared to be an exceedingly black cloud seemed to hang over all that part of it; yet it was no cloud, but only the serene sky, more than ordinary pure and limpid, so that the bright stars shone clearly in it."

The Doctor next mentions "two *laminæ*, or streaks" of light, "lying in a position from the north by east to the north east, and were each about a degree broad; the undermost about eight or nine degrees high, and the other about four or five degrees over it: these kept their places for a long time, and made the sky so light, that I believe a man might easily have read an ordinary print by the help thereof." And again: "It being now past eleven of the clock, and nothing new offering itself to our view but repeated *phases* of the same spectacle. I observed, that the two *laminæ* or streaks, parallel to the horizon, had now wholly disappeared; and the whole spectacle reduced itself to the resemblance of a very bright *crepusculum*, setting on the northern horizon, so as to be brightest and highest under the pole-star itself, from whence it spread both ways into the north-east and north-west."

About the time that this aurora appeared, the variation at London, the place of observation, was about 18° westward; and, consequently, neither the two steaks of light, nor the *crepusculum* in the north, had any relation to the magnetic meridian. The *nubecula* seen by Dr Halley, seems to have been of the same character as the flashes or waves of luminous vapour, seen at Prestwich during the auroræ of November 21st and December 17th last; they were obviously at no very great altitude, certainly within the range of aqueous vapour; the colour of these waves was that of a dim silvery whiteness.

I perfectly agree with Halley, Hansteen, Brewster, and many other eminent philosophers, in the belief of a magnetic element or effluvium, pervading the atmosphere, and perhaps all space; but the principles of Electro-magnetism do not allow of electric currents traversing the magnetic lines of force in the direction of their length, unless constrained by other influences than any known to exist in the regions of the aurora borealis. It is possible, however, that the theoretical views which I have here advanced may be open to objections that I do not myself perceive, and may require the corrections of a more diligent observer, and a sounder reasoner on the facts observed.

On a Formula for calculating the Expansion of Liquids by Heat. By WILLIAM JOHN MACQUORN RANKINE, Esq., Civil Engineer. Communicated by the Author.

Having been lately much engaged in researches involving the comparative volumes of liquids at various temperatures, I have found the following formula very useful:

$$\text{Log } V = B\,t + \frac{C}{t} - A$$

Log V represents the common logarithm of the volume of a given mass of liquid, as compared with its volume at a certain standard temperature, which, for water, is the temperature of its maximum density, or 4°·1 centigrade, and for other liquids 0° centigrade.

t is the temperature measured from the absolute zero mentioned in my paper on the Elasticity of Vapours, in the Edinburgh New Philosophical Journal for July 1849, and is found by adding 274$^{\text{c}}$·6 to the temperature according to the centigrade scale.

A, B, and C, are three constants, depending on the nature of the liquid, whose values for the centigrade scale, corresponding to water, mercury, alcohol, and sulphuret of carbon, are given below.

	A.	Log B.	Log C.
Water, . .	0·4414907	$\bar{4}$·8987546	1·7890286
Mercury, .	0·0229130	$\bar{5}$·9048766	$\bar{1}$·3703897
Alcohol, . .	0·2615033	$\bar{4}$·8414452	1·2893056
Sulphuret of Carbon,	0·2540074	$\bar{4}$·8483872	1·2192054

The data from which the constants have been computed have been taken from the following authorities:—for water, from the experiments of Hallström; for mercury, from those of Regnault; and for alcohol and sulphuret of carbon, from those of Gay-Lussac. As the experiments of M. Gay-Lussac give only the apparent expansion of the liquids in glass, I have assumed, in order to calculate the true expansion, that the dilatation of the glass used by him was ·0000258 of its volume for each centigrade degree. This is very nearly the mean dilatation of the different kinds of glass. M. Regnault has shewn that, according to the composition and treatment of glass, the coefficient varies between the limits ·000022 and ·000028.

Annexed are given tables of comparison between the results of the formula and those of experiment. The data from which the constants were calculated are marked with asterisks.

The table for water shews, that between 0° and 30° centigrade, the formula agrees closely with the experiments of Hallström, and that from 30° to 100° its results lie between those of the experiments of Gay-Lussac and Deluc.

The experiments of Gay-Lussac originally gave the apparent volume of water in glass, as compared with that at 100°.

They have been reduced to the unit of minimum volume by means of Hallström's value of the expansion between 4°·1 and 30°, and the coefficient of expansion of glass already mentioned.

In the fifth column of the table of comparison for mercury it is stated which of the experimental results were taken from M. Regnault's own measurements on the curve, representing the mean results of his experiments, and which from his tables of actual experiments, distinguishing the series.

In the experimental results for alcohol and sulphuret of carbon, the respective units of volume are the volumes of those liquids at their boiling points, and the volumes given by the formula have been reduced to the same units.

Expansion of Water.

Temperature on the Centigrade Scale.	Volume as compared with that at 4°·1 C. according to		Difference between Calculation and Experiment.	Authorities for the Experiments.
	the Formula.	the Experiments.		
0°	1·0001120	1·0001082	+·0000038	Hallström.
*4·1	1·0000000	1·0000000	·0000000	Do.
10	1·0002234	1·0002200	+·0000034	Do.
20	1·0015668	1·0015490	+·0000178	Do.
*30	1·0040245	1·0040245	·0000000	Do.
...	...	1·0041489	−·0001244	Deluc.
40	1·00750	1·00748	+·00002	Gay-Lussac.
...	...	1·00774	−·00024	Deluc.
60	1·01718	1·01670	+·00048	Gay-Lussac.
...	...	1·01773	−·00055	Deluc.
80	1·03007	1·02865	+·00142	Gay-Lussac.
...	...	1·03092	−·00085	Deluc.
100	1·04579	1·04290	+·00289	Gay-Lussac.
...	...	1·04664	−·00085	Deluc.

Expansion of Mercury.

Temperature on the Centigrade Scale.	Volume as compared with that at 0° C. according to		Difference between Calculation and Experiment.	Remarks.
	the Formula.	M. Regnault's Experiments.		
* 0°	1·000000	1·000000	·000000	Curve.
90·22	1·016333	1·016361	−·000028	Series I.
100·00	1·018134	1·018153	−·000019	Curve.
100·52	1·018230	1·018267	−·000037	Series I.
*150·00	1·027419	1·027419	·000000	Curve.
198·79	1·036597	1·036468	+·000129	Series II.
205·07	1·037786	1·037805	−·000019	Series IV.
205·57	1·037905	1·037910	−·000005	Series III.
*300·00	1·055973	1·055973	·000000	Curve.

Expansion of Alcohol.

Temperature on the Centigrade Scale.	Volume as compared with that at 78°·41 C. according to		Difference between Calculation and Experiment.
	the Formula.	M. Gay-Lussac's Experiments.	
3°·41	·91795	·91796	−·00001
*18·41	·93269	·93269	·00000
33·41	·94803	·94799	+·00004
*48·41	·96449	·96449	·00000
63·41	·98183	·98210	−·00027
*78·41	1·00000	1·00000	·00000

Expansion of Sulphuret of Carbon.

Temperature on the Centigrade Scale.	Volume as compared with that at 46°·60 C. according to		Difference between Calculation and Experiment.
	the Formula	M. Gay-Lussac's Experiments.	
* −13°·40	·93224	·93224	·00000
+ 1·60	·94768	·94776	−·00008
* 16·60	·96417	·96417	·00000
31·60	·98163	·98163	·00000
* 46·60	1·00000	1·00000	·00000

On the Geographical Distribution and Uses of the Common Oyster (Ostrea edulis.)

The *Ostrea edulis* may be said to have its capital in Britain; for though found elsewhere on the coasts of Europe, both northwards and southwards, in no part of them does it attain such perfection as in our seas, through which it is generally distributed, sparingly in some places, abundantly, and in gregarious assemblages in others, chiefly inhabiting the laminarian and coralline zones. The ancient Romans valued our native oysters even as we do now, and must have held them in higher estimation than those of Italian shores, or they would not have brought them from so far for their luxurious feasts.

In Bishop Spratt's "History of the Royal Society," is contained the first paper of importance on the Oyster-fisheries of England. It is selected by the Bishop as one of the examples which he gives of the various kinds of papers read before the Royal Society at that time, and respecting it he well remarks, "It may, perhaps, seem a subject too mean to be particularly alleged, but to me it appears worthy to be produced. For though the British oysters have been famous in the world ever since this island was discovered, yet the skill how to order them aright has been so little considered among ourselves that we see, at this day, it is confined to some narrow creeks of one single county." The paper is so short, concise, and important, in its bearing on the history of British oyster-fisheries, that we transcribe it nearly entire. It is entitled "The History of the Generation and Ordering of Green Oysters, commonly called Colchester Oysters," and runs thus:—"In the month of May the oysters cast their spawn (which the dredgers call their spat): it is like to a drop of caudle, and about the bigness of an halfpenny. The spat cleaves to stones, old oyster-shells, pieces of wood, and such like things, at the bottom of the sea, which they call cultch. It is probably conjectured that the spat, in twenty-four hours, begins to have a shell. In the month of May the dredgers (by the law of the Admiralty Court) have liberty to catch all manner of oysters of what size soever. When they have taken them, with a knife they gently raise the small brood from the cultch, and then they throw the cultch in again, to preserve the ground for the future, unless they be so newly spat that they cannot be safely severed from the cultch; in that case they are permitted to take the stone or shell, &c., that the spat is upon, one shell having, many times, twenty spats. After the month of May it is felony to carry away the cultch, and punishable to take any other oysters, unless it be those of size (that is to say) about the bigness of an half-crown piece, or when, the two shells being shut, a fair shilling will rattle between them. The places where these oysters are chiefly catched are called the Pont-Burnham, Malden, and Calne-water. * * * This brood, and other oysters, they carry to creeks of the sea at Brickelsea, Mersey, Langro, Fringrego, Wivenho, Tolesbury, and Saltcoase, and there throw them into the channel, which they call their beds or layers, where they grow and fatten, and, in two or three years, the smallest brood will be oysters of the size aforesaid. Those oysters which they would have green they put into pits about three foot deep, in the salt marshes, which are overflowed only at spring-tides, to which they have sluices, and let out the sea-water until it is about a foot-and-a-half deep. These pits, from some quality in the soil co-operating with the heat of the sun, will become green, and communicate their colour to the oysters that are put into them in four or five days, though they commonly let them continue there six weeks or two months, in which time they will be of a dark green. * * The oysters, when the tide comes in, lie with their hollow shell downwards, and when it goes out they turn on the other side; they remove not from their places unless in cold weather to cover themselves in the ooze. The reason of the scarcity of oysters, and consequently of their dearness, is, because they are of late years bought up by the Dutch. There are great penalties by the Admiralty Court laid upon those that fish out of those grounds which the Court appoints, or that destroy the cultch, or that take any oysters that are not of size, or that do not tread under their feet, or throw upon the shore a fish which they call a Five-finger, resembling a spur-rowel, because that fish gets into the oysters when they gape, and sucks them out. * * * The oysters are sick after they have their spat; but in June and July they begin to mend, and in August are perfectly well. The male oyster is black-sick, having a black substance in the fin; the female white-sick, having a milky substance in the fin. They are salt in the pits, salter in the layers, saltest at sea."

From this old paper the greater part of the matter contained in articles on the subject of oyster-fisheries in the several Encyclopædias has been derived. In the earlier volumes of the "Philosophical Transactions" are several notices on the subject of oysters, especially a short account of the spat by the celebrated Leuwenhoek, and a letter from the Rev. Mr Rowland to Dr Derham, in which it is stated that though the beds in the Menai furnished then (1720), as they do now, abundant oysters, twenty-four years previously none existed in the locality; they were originally laid down there by a private gentleman. These beds are now recruited from the Irish coast.

In order to obtain the most recent information respecting the oyster-beds which supply the London market, the extent of the supply, and the opinions of those practically concerned in their management, and in the sale of their products, on points in the history and value of what may be termed *culti-*

vated oysters, we drew up a series of queries, to which, chiefly through the obliging interest taken in the inquiry by Mr J. S. Sweeting, of 159 Cheapside, we have received from that gentleman, and from other well-informed quarters, very full replies, the results of which we now give in a condensed form.

The oyster-beds from which the principal supply for the London market is procured, are those of Whitstable, Rochester, Milton, Colchester, Burnham, Faversham, and Queenborough, all artificial beds, furnishing *natives*. Since the introduction of steamboats and railrods, considerable quantities of sea-oysters are brought from Falmouth and Helford in Cornwall, from the coast of Wales, the Isle of Wight, and neighbourhood of Sussex, and even from Ireland and Scotland, after the winter sets in, as before they would not keep fresh enough when brought from long distances. The supply derived from natural beds varies much, since on some of them the oysters are not sufficiently abundant to pay for dredging. The sea-oyster is often, before being brought to market, kept for a time in artificial beds, in order to improve its flavour.

The most esteemed oysters are those of the small, ovate, but deep-shelled variety, called *Natives*, among which those of the river Crouch, or Burnham oysters, are pre-eminent for their marine flavour, probably on account of the facilities for rapid importation of them in fine condition. Much of the quality depends on the ground and condition of the beds; and oysters of different years from the same place often vary very materially in this respect. They are considered full-grown for the market when from five to seven years old; sea-oysters, at four years. The age is shewn by the annual layers of growth, or "shoots," on the convex valve. Up to three or four years, each annual growth is easily observed, but after their maturity it is not so easy to count the layers. Aged oysters become very thick in the shell. In the neighbourhood of fresh water the oyster grows fast, and improves in body and flavour. The flavour is said by some to improve by shifting the oysters as they approach their full growth. Frost kills numbers; and when they are left dry at *low ebbs*, the run of fresh water from the land turns them what is called "foxy," of a brownish-red colour. They are sometimes seized with sickness during the spawning season, and considerable numbers may die. Much labour is required to keep the beds in good order, cleansed from shells and rubbish, star-fishes, barnacles, corallines, and sea-weed, which grow freely in the spring of the year. On the cleanliness of the ground, the prolific character of the bed, if the oysters breed there, depends. If carefully attended to, a bed may last any length of time; but if neglected, it will become overgrown with weed and buried in mud, so that it can only be reclaimed by restocking at a great expense, or is altogether destroyed. Artificial beds, for the purpose of keeping a supply at hand for the London market are said to have been commenced about the year 1700, by the Kent and Essex Companies of Dredgers. The oyster does not breed freely, often not at all, on artificial beds, so that they require to be constantly restocked; and when they do spawn under such circumstances, the fry are said seldom to come to perfection. On their natural grounds they spawn profusely during the season, *i. e.*, during the summer months. The developing spawn is technically called "spat."

The oyster has not a few enemies. Star-fishes, especially the *Uraster rubens*, and *Solaster papposa*, are supposed to do great injury to the beds; the dredgers call them Five-fingers. Whelks, called by the fishermen whelk-tingle, or sting-winkle,—are also said to do much damage,—perforate the shells with small holes, selecting especially those of from one to two years' growth. They are popularly supposed to strike directly for the heart of the oyster. That most curious sponge, the *Cliona*, perforates the shell in all directions, and directs its operations, with a wonderful symmetry, as we now know, through the curious investigations of Mr Albany Hancock. Milne-Edwards states, that in some places on the coast of France, the oyster-beds run a risk of being destroyed through the tube-constructing powers of certain annelides (*hermellæ*), becoming buried under masses of their curious habitations framed of agglutinated particles of sand.

In London, the chief consumption of common oysters is from the 4th of August to January, and of natives from October to March. The consumption is said to be greatest during the hottest months after the commencement of the oyster season; the warmer the weather, the more oysters are consumed. They are brought to market in craft of various sizes; they are packed in bulk closely in the hold; in some cases, a cask of salt-water is kept, from which to sprinkle them superficially. Those that come by rail are packed with their convex shells downwards in bags and barrels. From the boats they are transferred to the salesmen, who keep them in a little salt and spring water, and shift them every twelve hours. Some pretend to improve them by "feeding" them with oatmeal. Oysters, like other bivalves, live chiefly on infusoria. The quantity consumed annually in London varies in different seasons. One informant states 20,000 bushels of natives, and 100,000 bushels of common oysters, to be about the mark; another estimates the quantity sold in the season, from the 4th day of August to the 12th day of May, to be nearly 100,000 London bushels, each bushel being 3 Manchester or imperial bushels; and that about 30,000 bushels of natives are sold during the same period by various companies. During the season commencing on August 4, 1848, and ending May 12, 1849, Mr Wickenden estimates about 130,000 bushels of oysters to have been sold in London, though of that quantity about one-fourth was sent away to various parts of the United Kingdom and the Continent.

The oyster-fisheries are protected by legislative enactments. Various acts of Parliament have been passed for the better preservation of the oyster-beds, and prevention of trespass upon them. To steal oysters is a larceny;* to dredge on an oyster-bed unlawfully or wilfully, is being guilty of a misdemeanor, punishable by fine—the fine not to exceed £20—or imprisonment three calendar months. It is as well that ardent conchologists should know these (to them) obnoxious enactments, for otherwise they may find the search for a new or rare species, at the wrong season of the year, cost more trouble and expense than it is worth. It is not only artificial oyster-beds which are claimed as private property, but many of those in the open sea, on various parts of our coasts.

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* 31st Geo. III. c. 51; 48th Geo. III, c. 144; 7th and 8th Geo. IV., c. 29.

Oysters of good repute are fished in the neighbourhood of the Channel Islands. There are two oyster-banks, the one off Guernsey and the other off Jersey. The former is of little importance, the latter of considerable value. They belong to the region of oyster-banks which extends along the coasts of Normandy and Brittany. Dr Knapp informs us that the number procured annually from them, for the use of the Channel Islands and English markets, cannot be less than 800,000 tubs, each tub containing two English bushels; and in some years thrice that quantity is believed to be procured from those banks during the season. As many as three hundred cutters have been employed upon them dredging. The oysters on the Jersey bank are of large size, and are sold at from five to seven shillings the tub, or from three to four pence the dozen. Milne-Edwards and Audouin state (in their *Histoire Naturelle du Littoral de la France*), that, during the year 1828, the total number dredged on the French banks of this region was about 52,000,000, the average price of which was three francs fifty cents for every "miller," *i. e.*, twelve hundred. These French oyster-banks are stated, by the authors quoted, to yield a produce valued at from eight to nine hundred thousand francs a-year. Before the French oyster-fisheries were put under restrictions, the banks were deteriorating through continual fishing.

The oyster-fishery of most consequence in Scotland is that of the Frith of Forth, respecting which some valuable information has been communicated to us by Dr Knapp. The oyster-beds there extend about twenty miles, from the island of Mucra to Cockenzie, and are dredged in from four to six or seven fathoms water. The best are procured near Burntisland, on a bed belonging to the Earl of Morton,—on the rocky ground opposite Portobello,—and at Prestonpans. The price varies, wholesale, from two shillings to two shillings and sixpence per hundred; the retail price from two shillings and sixpence to four shillings and sixpence, or even five shillings. Eight or ten years ago the price was much less; but an individual having taken the ground off Newhaven for a high rent,—which he is said never to have paid,—so cleared the beds that they have since been comparatively rare.[*]

Natural oyster-beds, of small extent, occur at some distance from land in several places around the Isle of Man. The principal is that off Lascey; but, though the oysters are fine and well flavoured, their abundance is not sufficient to induce a regular fishery.

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[*] *Note on the Oyster-Fisheries which supply the Edinburgh Market. By Mr George D. Moffat.*—Twenty-five boats, working for four months, viz., September, October, March, and April, say sixty-four days (four days per week), dredge at an average 480 oysters per boat per day. *Inde,*

25 × 64 × 480 = ... 768,000

Eight boats, working for four months, viz., November, December, January, and February, say sixty-four days (four days per week) dredge at an average 480 oysters per day per boat. *Inde,*

8 × 64 × 480 = ... 245,760

Number of oysters dredged at an average in the season at Newhaven, ... 1,013,760

Fisherrow, Prestonpans, and Cockenzie, may be taken in, *all* at the same *ratio*. Therefore, doubling the above, makes 2,027,520 oysters, which may be calculated to be dredged in the Forth in the season; only three-fourth parts of which, however, it is believed are sent to Edinburgh, being 1,520,640.

From the foregoing average, the quantity dredged per day may be stated as follows:—

	Boats. Oysters.	
Principal season, four months,	25 × 480 = . .	12,000
Secondary season, four months,	8 × 480 = . .	3,840
	Per day, for Newhaven,	15,840

The same number for Fisherrow, Prestonpans, and Cockenzie, makes 31,680, three-fourth parts of which, as before mentioned, come to Edinburgh, being 23,760.

With regard to the consumption in Edinburgh, it will be apparent, that out of the season of eight months, only 128 days are stated, these being the men's working days. But the days of the consumption of these molluscs in town, are (excluding Sundays), out of eight months, 207 days. *Inde,* as before,

1,520,640 ÷ 207 = 7346 oysters, being the average daily consumed in Edinburgh during the season, from the beginning of September till the end of *April.*

On both sides of Ireland oysters abound in many places, and some of the banks are valuable, producing oysters in abundance, and of good quality. In the west, the most famous are Burton Bindon's oysters, which are highly esteemed in Dublin. They are the Burran oysters, brought from the Burran bank in Galway Bay, where they are laid down artificially, after having been originally dredged chiefly near Achil Head. There are oyster-beds in the Shannon, said, in 1836, to yield a revenue of £1400 annually, and to employ seventy men and sixteen boats. Some small oyster-beds in Clare are private property, and yield various incomes, as do those also in Cork harbour, but none of them are of any extent. Oysters are dredged from natural beds on the coast of Wexford and elsewhere, in order to be laid down on the Beaumaris beds. The most renowned of the Irish oyster-fisheries is that of Carlingford. The shell-fish are there dredged by boats, each manned by from three to five men, who take about fifty dozen a-day. The oysters of each boat are deposited within a ring of large stones till sold, the place being marked by a buoy. They are sold to dealers only, at from 8d. to 2s. per ten dozen. A yearly fee of 5s. is paid by each boat to the Marquis of Anglesey. The fishermen earn from 4d. to 1s. 6d. per diem, and are mostly landholders.*

There are natural oyster-beds in Belfast Bay, on banks at a depth of from 12 to 25 fathoms. Mr W. Thomson informs us that, in March 1848, he had the four largest oysters selected from about five hundred taken on these beds, and by weighing them before their being opened, found two to be each one pound and a-half, the third one pound and three-quarters, and the fourth two pounds imperial weight. "The two largest oysters," he states, "on being taken from their shells, weighed each an ounce and a-half, and the others somewhat less. The oysters from which these were selected were sold at the rate of sixteen shillings for the one hundred and twenty-four. The shells were in length from 5½ to 6½ inches; in breadth, from 5 inches to 5½; and in depth, with the valves closed, 2½ inches." There are oyster-beds partly private, and increased by planting in Loch Swilly. Irish oyster-dredgers have a notion that the more the banks are dredged, the more the oysters breed.*

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* Report on Irish Fisheries for 1836.

Comets—Great Number of Recorded Comets—The Number of those unrecorded probably much greater—General Description of a Comet—Comets without Tails, or with more than one—Their extreme Tenuity—Their probable Structure—Motions conformable to the Law of Gravity—Actual Dimensions of Comets—Great Interest at present attached to Cometary Astronomy, and its Reasons—Remarks on Cometary Orbits in general.

In the admirable Outlines of Astronomy, by Sir John F. W. Herschel, just published, where all is excellent, we were deeply interested with the account of those wonderful members of our system—the Comets. From this masterpiece of thought and writing, we now lay before our readers the following extracts :†—

The extraordinary aspect of comets, their rapid and seemingly irregular motions, the unexpected manner in which they often burst upon us, and the imposing magnitudes which they occasionally assume, have, in all ages, rendered them objects of astonishment, not unmixed with superstitious dread to the uninstructed, and an enigma to those most conversant with the wonders of creation, and the operations of natural causes. Even now, that we have ceased to regard their movements as irregular, or as governed by other laws than those which retain the planets in their orbits, their intimate nature, and the offices they perform in the economy of our m, are as much unknown as ever. No distinct and sa-tory account has yet been rendered of those immensely ninous appendages which they bear about with them, which are known by the name of their tails (though im-erly, since they often precede them in their motions), any e than of several other singularities which they present. he number of comets which have been astronomically erved, or of which notices have been recorded in history, ery great, amounting to several hundreds; and when we sider that, in the earlier ages of astronomy, and indeed more recent times, before the invention of the telescope, ly large and conspicuous ones were noticed, and that, since e attention has been paid to the subject, scarcely a year s passed without the observation of one or two of these dies, and that sometimes two, and even three, have ap-eared at once,—it will be easily supposed that their actual amber must be at least many thousands. Multitudes, in-eed, must escape all observation, by reason of their paths raversing only that part of the heavens which is above the horizon in the day-time. Comets so circumstanced can only become visible by the rare coincidence of a total eclipse of the sun,—a coincidence which happened, as related by Seneca, sixty-two years before Christ, when a large comet was actually observed very near the sun. Several, however, stand on record as having been bright enough to be seen with the naked eye in the day-time, even at noon and in bright sunshine. Such were the comets of 1402, 1532, and 1843, and that of 43 B.C. which appeared during the games celebrated by Augustus in honour of Venus shortly after the death of Cæsar, and which the flattery of poets declared to be the soul of that hero taking its place among the divinities.

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* The above observations are from Part XX. of Messrs Forbes and Stanley's valuable History of British Mollusca. To those interested in the natural and œconomic history of the common oyster, we recommend the perusal of a paper on the Danish Oyster-Beds, by M. H. Kroyer, at page 28 of vol. xxix. of this Journal.

† Outlines of Astronomy. By Sir John F. W. Herschel, Bart., &c. &c. &c. 1 vol. 8vo, pp. 661. London: Longman, Brown, Green, and Longmans; and J. *Taylor.* *1849.*

That feelings of awe and astonishment should be excited by the sudden and unexpected appearance of a great comet, is no way surprising [illegible], in fact, according to the accounts we have [illegible] one [illegible] the most im[illegible]sing of all natural [illegible] for t[illegible] part of [illegible] il[illegible] bu-[illegible] ed t[illegible] uch [illegible] centr[illegible] of a vivid *nucleus*, like a star or planet. From the head, and in a direction *opposite to that in which the sun is situated* from the comet appear to diverge two streams of light, which grow broader and more diffused at a distance from the head, and which most commonly close in and unite at a little distance behind it, but sometimes continue distinct for a great part of their course; producing an effect like that of the trains left by some bright meteors, or like the diverging fire of a sky-rocket (only without sparks or perceptible motion): This is the tail. This magnificent appendage attains occasionally an immense apparent length. Aristotle relates of the tail of the comet of 371 B.C., that it occupied a third of the hemisphere, or 60°; that of A.D. 1618, is stated to have been attended by a train no less than 104° in length. The comet of 1680, the most celebrated of modern times, and on many accounts, the most remarkable of all, with a head not exceeding in brightness a star of the second magnitude, covered with its tail an extent of more than 70° of the heavens, or, as some accounts state, 90°; that of the comet of 1769, extended 97°; and that of the last *great* comet (1843), was estimated at about 65° when longest. The figure (Fig. 2, Plate II.)* is a representation of the comet of 1819,—by no means one of the most considerable, but which was, however, very conspicuous to the naked eye.

The tail is, however, by no means an invariable appendage of comets. Many of the brightest have been observed to have short and feeble tails, and a few great comets have been entirely without them. Those of 1585 and 1763 offered no vestige of a tail; and Cassini describes the comets of 1665 and 1682 as being as round† and as well defined as Jupiter. On the other hand, instances are not wanting of comets furnished with many tails or streams of diverging light. That of 1744 had no less than six, spread out like an immense fan, extending to a distance of nearly 30° in length. The small comet of 1823 had two, making an angle of about 160°, —the brighter turned as usual from the sun; the fainter, towards it, or nearly so. The tails of comets, too, are often somewhat curved, bending, in general, towards the region which the comet has left, as if moving somewhat more slowly, or as if resisted in their course.

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* This refers to a Plate in the "Outlines," but not copied here.

† This description however applies to the "disc" of the head of these comets as seen in a telescope. Cassini's expressions are, "Aussi rond, aussi net, et aussi clair que Jupiter" (where, it is to be observed, that the latter epithet must by no means be translated *bright*). To understand this passage fully, the reader must refer to the description given further on, of the "disc" *of Halley's comet*, after its perihelion passage in 1835–6.

The smaller comets, such as are visible only in telescopes, or with difficulty by the naked eye, and which are by far the most numerous, offer very frequently no appearance of a tail, and appear only as round or somewhat oval vaporous masses, more dense towards the centre, where, however, they appear to have no distinct nucleus, or anything which seems entitled to be considered as a solid body. Stars of the smallest magnitudes remain distinctly visible, though covered by what appears to be the densest portion of their substance; although the same stars would be completely obliterated by a moderate fog, extending only a few yards from the surface of the earth. And since it is an observed fact, that even those larger comets which have presented the appearance of a nucleus have yet exhibited *no phases;* though we cannot doubt that they shine by the reflected solar light, it follows that even these can only be regarded as great masses of thin vapour, susceptible of being penetrated through their whole substance by the sunbeams, and reflecting them alike from their interior parts and from their surfaces. Nor will any one regard this explanation as forced, or feel disposed to resort to a phosphorescent quality in the comet itself, to account for the phenomena in question, when we consider (what will be hereafter shewn) the enormous magnitude of the space thus illuminated, and the extremely small *mass* which there is ground to attribute to these bodies. It will then be evident that the most unsubstantial clouds which float in the highest regions of our atmosphere, and seem at sunset to be drenched in light, and to glow throughout their whole depth as if in actual ignition, without any shadow or dark side, must be looked upon as dense and massive bodies, compared with the filmy and all but spiritual texture of a comet.

Accordingly, whenever powerful telescopes have been turned on these bodies, they have not failed to dispel the illusion which attributes *solidity* to that more condensed part of the head, which appears to the naked eye as a nucleus; though it is true that in some a very minute stellar point *has* been seen, indicating the existence of a solid body.

It is in all probability to the feeble coercion of the elastic power of their gaseous parts, by the gravitation of so small a central mass, that we must attribute this extraordinary development of the atmospheres of comets. If the earth, retaining its present size, were reduced, by any internal change (as by hollowing out its central parts) to one-thousandth part of its actual mass, its coercive power over the atmosphere would be diminished in the same proportion, and, in consequence, the latter would expand to a thousand times its actual bulk; and indeed much more, owing to the still further diminution of gravity, by the recess of the upper parts from the centre.* An atmosphere, however, free to expand equally in all directions, would envelope the nucleus spherically, so that it becomes necessary to admit the action of other causes to account for its enormous extension in the direction of the tail,—a subject to which we shall presently take occasion to recur.

That the luminous part of a comet is something in the nature of a smoke, fog, or cloud, suspended in a transparent atmosphere is evident from a fact which has been often noticed, viz., that the portion of the tail where it comes up and surrounds the head, is yet separate from it by an interval less luminous, as if sustained and kept off from contact by a transparent stratum, as we often see one layer of clouds over another, with a considerable clear space between. These, and most of the other facts observed in the history of comets, appear to indicate that the structure of a comet, must be that of a hollow envelope, of a parabolic form, enclosing near its vortex the nucleus and head. This would account for the apparent division of the tail into two principal lateral branches, the envelope being oblique to the line of sight at its borders, and therefore a greater depth of illuminated matter being there exposed to the eye. In all probability, however, they admit great varieties of structure, and among them may very possibly be bodies of widely different physical constitution, and there is no doubt that one and the same comet at different epochs undergoes great changes, both in the disposition of its materials and in their physical state.

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* Newton has calculated (*Princ.* iii., p. 512) that a globe of air of ordinary density at the earth's surface, of one inch in diameter, if reduced to the density due to the altitude above the surface of one radius of the earth, would occupy a sphere exceeding in radius the orbit of Saturn. The tail of a great comet, then, for aught we can tell, may consist of only a very few pounds or even ounces of matter.

We come now to speak of the motions of comets. These are apparently most irregular and capricious. Sometimes they remain in sight only for a few days, at others for many months; some move with extreme slowness, others with extraordinary velocity; while not unfrequently the two extremes of apparent speed are exhibited by the same comet in different parts of its course. The comet of 1472 described an arc of the heavens of 40° of a great circle* in a single day. Some pursue a direct, some retrograde, and others a tortuous and very irregular course; nor do they confine themselves, like the planets, within any certain region of the heavens, but traverse indifferently every part. Their variations in apparent size, during the time they continue visible, are no less remarkable than those of their velocity; sometimes they make their first appearance as faint and slow-moving objects, with little or no tail; but by degrees accelerate, enlarge, and throw out from them this appendage, which increases in length and brightness till (as always happens in such cases) they approach the sun, and are lost in his beams. After a time they again emerge, on the other side, receding from the sun with a velocity at first rapid, but gradually decaying. It is for the most part after thus passing the sun, that they shine forth in all their splendour, and that their tails acquire their greatest length and development; thus indicating plainly the action of the sun's rays as the exciting cause of that extraordinary emanation. As they continue to recede from the sun, their motion diminishes, and the tail dies away, or is absorbed into the head, which itself grows continually feebler, and is at length altogether lost sight of, in by far the greater number of cases never to be seen more.

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* 120° in extent in the former editions; but this was the arc described in *longitude*, and the comet at the time referred to had great north latitude.

Without the clue furnished by the theory of gravitation, the enigma of these seemingly irregular and capricious movements might have remained for ever unresolved. But Newton, having demonstrated the possibility of any conic section whatever being described about the sun, by a body revolving under the dominion of that law, immediately perceived the applicability of the general proposition to the case of cometary orbits; and the great comet of 1680, one of the most remarkable on record, both for the immense length of its tail and for the excessive closeness of its approach to the sun (within one-sixth of the diameter of that luminary), afforded him an excellent opportunity for the trial of his theory. The success of the attempt was complete. He ascertained that this comet described about the sun as its focus an elliptic orbit of so great an excentricity as to be undistinguishable from a parabola (which is the extreme, or limiting form of the ellipse when the axis becomes infinite), and that in this orbit the areas described about the sun were, as in the planetary ellipses, proportional to the times. The representation of the apparent motions of this comet by such an orbit, throughout its whole observed course, was found to be as satisfactory as those of the motions of the planets in their nearly circular paths. From that time it became a received truth, that the motions of comets are regulated by the same general laws as those of the planets,—the difference of the cases consisting only in the extravagant elongation of their ellipses, and in the absence of any limit to the inclinations of their planes to that of the ecliptic,—or any general coincidence in the direction of their motions from west to east, rather than from east to west, like what is observed among the planets.

It is a problem of pure geometry, from the general laws of elliptic or parabolic motion, to find the situation and dimensions of the ellipse or parabola which shall represent the motion of any given comet. In general, three complete observations of its right ascension and declination, with the times at which they were made, suffice for the solution of this problem, (which is, however, by no means an easy one), and for the determination of the elements of the orbit. These consist, *mutatis mutandis*, of the same data as are required for the computation of the motion of a planet; (that is to say, the longitude of the perihelion, that of the ascending node, the inclination to the ecliptic, the semi-axis, excentricity, and time of perihelion passage, as also whether the motion is direct or retrograde); and, once determined, it becomes very easy to compare them with the whole observed course of the comet, by a process exactly similar to that of Art. 502 of this work, and thus at once to ascertain their correctness, and to put to the severest trial the truth of those general laws on which all such calculations are founded.

For the most part, it is found that the motions of comets may be sufficiently well represented by parabolic orbits,—that is to say, ellipses whose axes are of infinite length, or, at least, so very long that no appreciable error in the calculation of their motions, during all the time they continue visible, would be incurred by supposing them actually infinite. The parabola is that conic section which is the limit between the ellipse on the one hand, which returns into itself, and the hyperbola on the other, which runs out to infinity. A comet, therefore, which should describe an elliptic path, however long its axis, must have visited the sun before, and must again return (unless disturbed) in some determinate period,—but should its orbit be of the hyperbolic character, when once it had passed its perihelion, it could never more return within the sphere of our observation, but must run off to visit other systems, or be lost in the immensity of space. A very few comets have been ascertained to move in hyperbolas,* but many more in ellipses. These latter, in so far as their orbits can remain unaltered by the attraction of the planets, must be regarded as permanent members of our system.

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* For example, that of 1723, calculated by Burckhardt; that of 1771, by both Burckhardt and Encke; and the second comet of 1818, by Rosenberg and Schwabe.

We must now say a few words on the actual dimensions of comets. The calculation of the diameter of their heads, and the lengths and breadths of their tails, offers not the slightest difficulty when once the elements of their orbits are known, for by these we know their real distances from the earth at any time, and the true direction of the tail, which we see only foreshortened. Now calculations instituted on these principles lead to the surprising fact, that the comets are by far the most voluminous bodies in our system. The following are the dimensions of some of those which have been made the subjects of such inquiry.

The tail of the great comet of 1680, immediately after its perihelion passage, was found by Newton to have been no less than 20,000,000 of leagues in length, and to have occupied only two days in its emission from the comet's body, a decisive proof this of its being darted forth by some active force; the origin of which, to judge from the direction of the tail, must be sought in the sun itself. Its greatest length amounted to 41,000,000 leagues, a length much exceeding the whole interval between the sun and the earth. The tail of the comet of 1769 extended 16,000,000 leagues, and that of the great comet of 1811, 36,000,000. The portion of the head of this last, comprised within the transparent atmospheric envelope which separated it from the tail, was 180,000 leagues in diameter. It is hardly conceivable, that matter once projected to such enormous distances, should ever be collected again, by the feeble attraction of such a body as a comet—a consideration which accounts for the surmised progressive diminution of the tails of such as have been frequently observed.

We have been somewhat diffuse on the subject of this comet,* for the sake of shewing the degree and kind of interest which attaches to cometic astronomy in the present state of the science. In fact, there is no branch of astronomy more replete with interest, and we may add more eagerly pursued at present, inasmuch as the hold which exact calculation gives us on it may be regarded as completely established; so that whatever may be concluded as to the motions of any comet which shall henceforward come to be observed, will be concluded on new grounds and with numerical precision; while the improvements which have been introduced into the calculation of cometary perturbation, and the daily increasing familiarity of numerous astronomers with computations of this nature, enable us to trace their past and future history with a certainty, which, at the commencement of the present century could hardly have been looked upon as attainable. Every comet newly discovered is at once subjected to the ordeal of a most rigorous inquiry. Its elements, roughly calculated within a few days of its appearance, are gradually approximated to as observations accumulate, by a multitude of ardent and expert computists. On the least indication of a deviation from a parabolic orbit, its elliptic elements become a subject of universal and lively interest and discussion. Old records are ransacked, and old observations reduced, with all the advantage of improved data and methods, so as to rescue from oblivion the orbits of ancient comets which present any similarity to that of the new visitor. The disturbances undergone in the interval by the action of the planets are investigated, and the past, thus brought into unbroken connexion with the present, is made to afford substantial ground for prediction of the future. A great impulse meanwhile has been given of late years to the discovery of comets by the establishment in 1840,* by His late Majesty the King of Denmark, of a prize medal to be awarded for every such discovery, to the first observer (the influence of which may be most unequivocally traced in the great number of these bodies which every successive year has added to our list), and by the circulation of notices, by special letter,† of every such discovery (accompanied, when possible, by an ephemeris), to all observers who have shewn that they take an interest in the inquiry, so as to ensure the full and complete observation of the new comet, so long as it remains within the reach of our telescopes.

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* *This* refers to the Author's Account of the Great Comet of 1843, which we *have not,* from want of space, extracted.—Edit.

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* See the announcement of this Institution in Astron. Nach. No. 400.

† By Prof. Schumacher, Director of the Royal Observatory of Altona.

It is by no means merely as a subject of antiquarian interest, or on account of the brilliant spectacle which comets occasionally afford, that astronomers attach a high degree of importance to all that regards them. Apart even from the singularity and mystery which appertain to their physical constitution, they have become, through the medium of exact calculation, unexpected instruments of inquiry into points connected with the planetary system itself, of no small importance. We have seen that the movements of the comet of Encke, thus minutely and perseveringly traced by the eminent astronomer whose name is used to distinguish it, has afforded ground for believing in the presence of a resisting medium filling the whole of our system. Similar inquiries prosecuted in the cases of other periodical comets, will extend, confirm, or modify our conclusions on this head. The perturbations, too, which comets experience in passing near any of the planets, may afford, and have afforded, information as to the magnitude of the disturbing masses, which could not well be otherwise obtained. Thus the approach of this comet to the planet Mercury in 1838 afforded an estimation of the mass of that planet the more precious, by reason of the great uncertainty under which all previous determinations of that element laboured. Its approach to the same planet in the present year (1848) will be still nearer. On the 22d of November their mutual distance will be only fifteen times the moon's distance from the earth. It is, however, in a physical point of view that these bodies offer the greatest stimulus to our curiosity. There is, beyond question, some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the etherial or imponderable element), may *ere long* enable us to penetrate this mystery, and to declare whether it is really *matter* in the ordinary acceptation of the term which is projected from their heads with such extravagant velocity, and if not impelled, at least *directed* in its course by a reference to the sun, as its point of avoidance. In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration, than in that of the enormous sweep which it makes round the sun *in perihelio*, in the manner of a straight and rigid rod, in defiance of the law of gravitation, nay, even of the received laws of motion, extending (as we have seen in the comets of 1680 and 1843) from near the sun's surface to the earth's orbit, yet whirled round unbroken; in the latter case through an angle of 180° in little more than two hours. It seems utterly incredible that in such a case, it is one and the same material object which is thus brandished. If there could be conceived such a thing as a *negative shadow*, a momentary impression made upon the luminiferous ether behind the comet, this would represent, in some degree the conception such a phenomenon irresistibly calls up. But this is not all. Even such an extraordinary excitement of the ether, conceive it as we will, will afford no account of the projection of lateral streamers; of the effusion of light from the nucleus of a comet towards the sun; and its subsequent rejection; of the irregular and capricious mode in which that effusion has been seen to take place; none of the clear indications of alternate evaporation and condensation going on in the immense regions of space occupied by the tail and coma,—none, in short, of innumerable other facts which link themselves with almost equally irresistible cogency to our ordinary notions of matter and force.

The great number of comets which appear to move in parabolic orbits, or orbits at least undistinguishable from parabolas during their description of that comparatively small part within the range of their visibility to us, has given rise to an impression that they are bodies extraneous to our system, wandering through space, and merely yielding a local and temporary obedience to its laws during their sojourn. What truth there may be in this view, we may never have satisfactory grounds for deciding. On such an hypothesis, our elliptic comets owe their permanent denizenship within the sphere of the sun's predominant attraction to the action of one or other of the planets near which they may have passed, in such a manner as to diminish their velocity, and render it compatible with elliptic motion.* A similar cause acting the other way, might with equal probability, give rise to a hyperbolic motion. But whereas in the former case, the comet would remain in the system, and might make an indefinite number of revolutions, in the latter it would return no more. This may possibly be the cause of the exceedingly rare occurrence of a hyperbolic comet as compared with elliptic ones.

All the planets without exception, and almost all the satellites, move in one direction round the sun. Retrograde comets, however, are of very common occurrence, which certainly would go to assign them an exterior or at least an independent origin. Laplace, from a consideration of all the cometary orbits known in the earlier part of the present century, concluded, that the mean or average situation of the planes of all the cometary orbits, with respect to the ecliptic, was so nearly that of perpendicularity, as to afford no presumption of any cause biassing their directions in this respect. Yet we think it worth noticing that among the comets which are as yet known to describe elliptic orbits, not one whose inclination is under 17° is retrograde ; and that out of thirty-six comets which have had elliptic elements assigned to them, whether of great or small excentricities, and without any limit of inclination, only five are retrograde, and of these only two, viz., Halley's and the Great Comet of 1843, can be regarded as satisfactorily made out. Finally, of the 125 comets whose elements are given in the collection of Schumacher and Olbers, up to 1823, the number of retrograde comets under 10° of inclination is only two out of nine, and under 20°, seven out of twenty-three. A plane of motion therefore, nearly coincident with the ecliptic, and a periodical return, are circumstances eminently favourable to direct revolution in the cometary as they are decisive among the planetary orbits.

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* The velocity in an ellipse is always less than in a parabola, at equal distances from the sun; in a hyperbola always greater.

On Oceanic Infusoria, Living and Fossil.

(Concluded from p. 160.)

But, perhaps, the most remarkable fact observed, is the result of soundings continued for 400 miles along the Victoria Barrier, where the existence of a bank, of unknown thickness, but, at least, of the extent of surface stated, was found, composed almost wholly of skeletons of these microscopic vegetables. Nothing else come up with the lead. Here, then, was a submarine deposit in process of formation, equalling, in extent, any similar deposit of the earlier world. Such strata are doubtless in course of accumulation in most parts of the ocean, and may be observed on our own shores; but this antarctic bank is the grandest example of the kind which has been carefully investigated by an able artist. But it is not only the sea and the land which yield the relics of these plants; the Diatomaceæ perform long journeys through the air. This remarkable fact rests on the authority of the accurate Darwin, who collected, at sea, small dust, which fell from the atmosphere on the planks and rigging of the ship, which dust, when examined with the microscope, was found composed of *Diatomaceæ*. These were on their flight from America to Africa. From their siliceous nature, they resist even the strong heat of volcanoes, and their remains are found thrown up in the pumice and dust from the crater. In fact, it is difficult to name a nook on the face of the earth, or in the depths of the sea, where they are wholly absent, either in a dead or living state; and their office, in the general economy, besides affording food for the humble members of the animal kingdom, seems to be the preparation of a soil for a higher class of vegetables. This they effect by the minute division of the siliceous particles laid up in their tissues, and probably make this nearly insoluble earth more fit for assimilation by other plants. We must also suppose them endowed, like other vegetables, with the power of decomposing carbonic acid, and liberating oxygen; and thus, in their countless myriads, exercising no mean place in the household of Nature. Like their mistress, these, her humblest servants, work in secret. We know not what we owe them. But, continued as their existence is through all time, and dispersed as they are through every part of the world,—even where the ice-bound sea is peopled by nothing else,—we may rest assured that they do perform some work which renders them worthy the care of a Providence who creates nothing superfluous. I have spoken of the *Diatomaceæ* as vegetables. Ehrenberg and many other writers regard them as infusorial animals; and, indeed, they have been bandied about from the animal to the vegetable kingdom at various times, according to the views of different naturalists. Latterly, the evidence seems to have preponderated on the vegetable side, especially since the brilliant discoveries of Mr Thwaites,* communicated to a late meeting of the British Association, have shewn that their fructification is precisely analogous to that of some of the lower algæ, and that the fruit resembles a spore.

A similar mode of fruiting is now discovered among Desmidieæ, which were also classed with infusoria by Ehrenberg, and, of these, a large number, in fruit, are figured in the work of Mr Ralfs, before alluded to; but, as they are natives of fresh water, it is out of place to enter on their history here. I may, however, remark, that the curious spiny bodies found fossilized in flint, which often pass for *Xanthidia*, are now proved to be only the spores of various genera of *Desmidieæ*, whose full-grown fronds are amazingly unlike the spore in form. The mode of forming fruit in both these families, *Desmidieæ* and *Diatomaceæ*, which is also the mode among undoubted algæ, is by the coupling together of two cells or frustules, when a passage is gradually formed between them, through which the contents of one cell are discharged into the other, where a dense mass of granular matter collects, which, at length, solidifies into a spore, and bursts through the walls of the cell. As such a process of reproduction is more analogous to what takes place in the vegetable than in the animal kingdom, naturalists seem now generally agreed to class them with vegetables. The advocates for their animal nature appeal to certain motions, having the character of voluntary motion, observed in many species. Thus, *Bacillaria paradoxa* alternately propels its frustules forward, and draws them back, opening out the filament of which the compound body consists into a straight line, and contracting it again into a narrow compass. This little plant resembles a pack of narrow cards, joined together at one of the angles of their smaller end; when extended, they are ranged in a straight line, and when contracted, they are folded back on each other, and lie as if in a pack. It is highly curious to watch the regular manner in which this motion is continued. Some of the other species have movements of a similar character, but many have not been observed in motion; and such motions as are seen, more resemble the regulated movement of a machine than the voluntary changes of place which animals exhibit. No doubt it is difficult, perhaps impossible, to draw a rigid line between the irritability of a vegetable and the muscular and nervous contractions of an animal, when we come to investigate such minute organisms as those we are now considering; but it is, at least, certain that mere motion, such as has been observed in the Diatomaceæ, is no proof of animality. And as the other points in their history ally them to the vegetable kingdom, the fact of their vegetability, if not quite proved (as I believe it to be), is, at least, extremely probable.

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* See Thwaites, in "An. Nat. Hist.," N. S., vol. i., p. 162, &c.

Before dismissing the subject of microscopic vegetables, I may remark, that the colouring of the waters of the Red Sea is now generally supposed to be caused by the presence of countless multitudes of a minute alga, which is perfectly invisible to the naked eye, except when great numbers are congregated together. Some writers have denied that the water of the Red Sea has any peculiar colour, or that its name is owing to the colour of its waters. Others, on the contrary, describe a red shade, of a very singular character, as present, and various explanations of the phenomena have been given. The differences among travellers in their account of this sea may be reconciled by supposing their observations to have been made at different seasons of the year; for, if the colour of the water depends on the presence of the vegetable matter, it is highly probable that it will vary in degree at different seasons. That its waters are occasionally coated with a scum of a red colour is certain, and portions of it have been brought home and carefully examined by several naturalists. M. Montagne has given an elaborate account of specimens which he had received, and has proved that the scum is entirely made up of a very minute alga, which consists of delicate threads, collected in bundles, and contains rings of a red matter, within a slender tube. This little plant has a structure very similar to the Oscillatoriæ, which form green scums on stagnant pools; or perhaps it more nearly resembles the pretty little fresh water alga, called (by the somewhat jaw-breaking name of) Aphanizomenon. Minute algæ of this description are by no means confined to the waters of the Red Sea, but are met with in many parts of the ocean, sometimes extending in broad bands for hundreds of miles. Mr Darwin, in his interesting Voyage, gives an acccount of several extraordinary bands of this description which he met with in the Pacific Ocean. I have had the advantage of inspecting some of the specimens brought home by this naturalist. They are very similar to the species of the Red Sea.—(*The Sea-Side Book, by Harvey,* p. 174.)

Notice of Land-Shells found beneath the surface of Sand-hillocks on the Coasts of Cornwall.[*] By RICHARD EDMONDS Junior, Esq.

In my Paper on the origin of our sand-hillocks, read before the Society in 1846,[†] it is stated that in one of the deep cuttings in the Towans of Phillack, within the space of a few inches, was found a great number of the shells of the *Helix pulchella,* a species now seldom met with in the living state in the west of Cornwall; and it was remarked by the Author of the *Cornish Fauna*, that "should further research shew they are of frequent occurrence in other parts of the Towans, we must come to the conclusion that they were once abundant in Cornwall but are now gradually becoming extinct in this locality." To ascertain the fact, I again went thither last summer with my nephew and two other young conchologists; and in the short space of two hours we picked up (amongst numerous other species) hundreds of these little shells in different parts of the sands, at depths varying from 1 foot to 30: they appeared almost as numerous, and as generally distributed throughout the hillocks, beneath the surface, as any species we saw. The *Zua lubrica* and some species of *Pupa* were also very abundant, particularly the *Pupa marginata.* We had not leisure to examine the surface for living individuals of any of the smaller species; but amongst the larger, we observed the *Helix aspersa, virgata, ericetorum, caperata,* and *fusca*, and the *Bulimus acutus.*

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[*] Transactions of the Royal Geological Society of Cornwall for 1848, p. 70. Read 5th October 1848.

[†] See Jameson's Journal for July 1847, p. 181.

The *exuviæ* of the *Helix pulchella* we found also in the sand-hills of Whitesand Bay, and in those near Gunwalloe and Mullion in Mounts Bay; they are abundant, too, in the sand-hills of Gorran, on the southern coast of East Cornwall; where also many living specimens have been observed on the surface. The last-mentioned sand-hills have been frequently visited by Mr Peach, to whom fossil geology is so much indebted; and his observations on them, at the last meeting of this Society, confirm the hypothesis suggested in my former Paper respecting the origin of our sand-hillocks.

The following is a list of the land-shells which we found last summer beneath the surface of the Phillack Towans; to which is added the *Zonites pygmæus* that we found in the Whitesand Bay Towans. Specimens of these 27 species are deposited at the Museum of the Penzance Natural History and Antiquarian Society, with the exception of the *Conovulus bidentatus*, which was accidentally lost after being brought home. Those marked with asterisks are not now found in the living state within ten miles of Penzance.—

Bulimus acutus.
——— obscurus.
Carychium minimum.
Clausilia biplicata.
Conovulus bidentatus.
——— denticulatus.
Helix aspersa.
——— caperata.
——— ericetorum.
——— fulva.*
——— fusca.
——— hortensis.
——— nemoralis.
——— pulchella.
Helix virgata.
Pupa Anglica.
——— marginata.*
——— umbilicata.
Vertigo edentula.
——— palustris.*
——— pygmæa.*
Vitrina pellucida.
Zonites alliarius.
——— cellarius.
——— nitidulus.
——— pygmæus.*
——— rotundatus.

PENZANCE, 6th October 1848.

On the Geographical Distribution of the Languages of Abessinia and the Neighbouring Countries. By CHARLES T. BEKE, Esq., Ph.D., F.S.A., &c.* (With a Map.) Communicated by the Ethnological Society.

A consideration of the Report on the Languages of Africa, made by Dr Latham to the British Association for the Advancement of Science, at the Meeting at Oxford in 1847,† has led me to believe that an acceptable addition to that Report will be afforded by the accompanying map, showing the approximate geographical limits of the several classes of languages spoken in Abessinia and the neighbouring countries, according to that philologist's provisional classification of them.

These languages are made by Dr Latham to consist of the following groups or classes:—

XIV. The Nubian.
XV. The Fátsoḳl (Fazoglo).
XVI. The Bishárye or Bidja (Beja).
XVII. The Ethiopic.
XVIII. The Agau (Agow).
XIX. The Galla.
XX. The Gonga.
XXI. The Shánkạla.
XXII. The Dalla.
XXIII. The Takue (or Bodje,) and Bárea.

Upon each of these classes I propose to make a few brief remarks, in explanation of the map.

XIV. *The Nubian Class.* These languages belong to countries which are partially shown, merely for the purpose of defining the limits of those which are subsequently enumerated. They are themselves too remote from Abessinia Proper, to come within the scope of the present observations.

XV. *The Fátsoḳl Languages.* In this class Dr Latham places only the languages of Fátsoḳl and Kamámil. From the immediate contiguity of the Gíndjar of Abu-Ramla and El 'Atiah,* I would suggest the probability that this language belongs to the same class. In the second volume of the Proceedings of the Philological Society,† I have given forty-two words of the Gíndjar tongue. Of these the greater number are evidently a corrupt Arabic; but the following eleven words may be regarded as *native* expressions:—

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* Read before the Section of Ethnology of the British Association for the Advancement of Science, at the Meeting at Swansea, on the 14th August 1848; and before the Ethnological Society of London, on the 22d November 1848.

† See *Report of the Seventeenth Meeting at Oxford*, 1847, p. 154, *et seq.*

earth	*wota.*	mouth	*shamạk.*
grass	*gesh.*	nose	*náhhera.*
mountain	*gạllah.*	bread	*kissera.*
boy	*djénna.*	good	*sạmmí.*
leg	*kuráh.*	bad	*fạssil.*
foot	*káfat kurái.*		

XVI. *The Bisharye or Bidja (Beja) Language.* The probable affinities of this tongue are stated by Dr Latham, on the authority of Dr Lepsius, to be with the Coptic; but, at the same time, the language of Suákin, which is classed with it, is said to have affinities with the Argóbba of Abessinia. As this latter dialect belongs to the Ethiopic Class (XVII.), it would seem that the Suákin language ought to be ranged under the same head.

XVII. *The Ethiopic Class of Languages.* This class comprehends the Tigre, Arkíko, Amhára, Argóbba, Hạrrargie (Hurrur) or Adhári, Gurágie, and Gáfat. I cannot agree to Dr Latham's proposition, in accordance with the opinion generally entertained, that these languages are the original ones of Abessinia. I can scarcely admit them to be those of the greater part of the country. On the contrary, I look upon the Agau languages (XVIII.) as holding a higher rank in the former respect, and probably in the latter likewise. The Geez, which is the ancient language of Tigre,—the most north-easterly province next the coast,—is that of the religion and literature of the country; and, when Tigre was dominant, it was that of the court. The Amharic, which is spoken in the south-east, is that of the present dominant race, and is used by the court, the army, and the merchants. It is that, too, with which alone travellers, who penetrate into Abessinia beyond Tigre, have ordinarily occasion to become familiar. But the Agau, in its various dialects, is the language of the *people*; in some provinces almost exclusively, and in others, where it has been superseded by the language of the dominant race, it still exists among the lowest classes; some of which, such as the Zaláns, Kamáunts, Wáitos, &c., may, from their entire separation from the other races, be looked on in the light of *castes*.

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* See *Journal of the Royal Geographical Society*, vol. xiv. p. 9.

† P. 95.

From the affinity of the Geez, Amharic, and cognate dialects, to the Arabic, it is reasonable to consider that they were introduced by conquerors or settlers from the opposite shores of the Red Sea. The traditions of the country are decidedly in favour of such an origin.

XVIII. *The Agau Languages.* The remarks made on the preceding class, render any additional ones unnecessary here.

XIX. *The Galla Class of Languages.* These are spoken by other intrusive people from the south, who have surrounded, and in part overrun Abessinia at a comparatively recent period. Their advance, which has been great and is still going on, is not so apparent as it might be, owing to the fact that, in many cases, the Gallas have adopted the language of the people whose place they have usurped. The Galla element is, however, fast becoming the predominant one in Abessinia. At the present day almost every principal ruler throughout the empire is, in the male line, of Galla extraction.

XX. *The Gonga Class of Languages.* To the languages and dialects of this class already furnished by myself,* one or two will probably have to be added from the vocabularies which M. d'Abbadie informs us he has collected. It is, however, expedient to defer the consideration of these additions, till after Dr Latham's list has been gone through.

XXI. *The Shánkạla Language.* Dr Latham identifies the language of Dabanja of the Mithridates with my Shánkạla of Agaumider. The chief place of the province of Agaumider is *Bándja*,† which name is apparently identical with Dabanja (=Dar-Bándja). The inhabitants of this province say that it was formerly inhabited by the Shánkạlas, whom they subdued and drove westwards into the valleys of the Blue River and its tributary streams.* Dr Latham suggests the restriction of the name of *Shánkqla* to the negroes of the low country bordering on Agaumider, to the exclusion of those of the valley of the Tạ́kkạzie. A recent traveller, M. Russegger, informs us,† that one of the tribes of the valley of the Blue River is called *Shangollo*, and he protests against the general application of the name Shangollo or Shánkạla to all the black people inhabiting the low lands surrounding Abessinia to the west and north. Whatever may be the derivation of the word, it is certain that among the Abessinians it has become an appellative, signifying "negro;" and hence it is applied by them, though as it would seem improperly, to the black people of the north of Abessinia—the "Shánkạlas of the Tạ́kkạzie"—who do not at present appear to have any affinity with the true Shánkạlas of the south-west.

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* *Proceedings of the Philological Society*, vol. ii., pp. 97–107.

† See *Journal of the Royal Geographical Society*, vol. xiv. p. 7.

It has yet to be ascertained whether the tongue of these true Shánkạlas of Agaumider is cognate with the languages of Class XV., with which it is conterminous.

XXII. *The Dalla Language* is that of the "Shánkạlas of the Tạ́kkạzie," above referred to.

XXIII. *The Takue* (*or Boje*) *and Bárea.* These languages are stated by Dr Latham to have affinities equally with the Dalla (XXII.) and with the Agau (XVIII.). This might be inferred from the position of the country in which they are spoken. But this alleged affinity with the Agau tongue suggests the probability, that, when we shall have acquired a more intimate knowledge of the languages and dialects of the tribes skirting Abessinia to the north and west,—the Shánkạlas of Agaumider, the Dalla, and the Takue,—we shall find these people to be, all of them, offsets from the aboriginal race of Abessinia, the Agaus, who have retired from the high tableland into the valleys of the rivers, before the advances of intruders of the Ethiopic class.

In the Amharic language, the word *Bárea* means "slave;" that is to say, a "Shánkạla," *negro*, or black slave—since, in the estimation of the Abessinians, the negroes, as the alleged descendants of Ham, are alone doomed to slavery. It seems, therefore, that "Bárea" is only another name for the Dalla negroes or "*Shánkạlas* of the Tạ́kkạzie."

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* *Journal of the Royal Geographical Society*, vol. xiv. p. 10.

† *Reise in Europa, Asien und Afrika*, vol. ii. part 2, p. 231.

In terminating his list of the languages of Abessinia, Dr Latham inquires, "Is it exhaustive?" and he refers to a number of vocabularies mentioned by M. d'Abbadie in the *Athenæum* of April 12, 1845,* as having been collected by him during his residence in that country. As even now we possess nothing more of these vocabularies than their *names*, we must be content to investigate them from the names alone. These are, however, quite sufficient to show, that, when the vocabularies themselves are given to the public, they are likely to present but little novelty, whatever value they may, and doubtless do, otherwise possess.

The languages to which M. d'Abbadie's vocabularies relate are stated to be as follows :—

A. THE AGAU (AGOW) LANGUAGES.

1. The Khamtiña (Khamtinga), "spoken by the *Khamta* or Agaus of Way or *Wag*." This is manifestly my *Waag*-Agau or *Hhámạra*,—the Agow of Bruce and Salt. (Latham, XVIII. 1, 2, 3.)

2. The Auña (Awnga), "spoken by the *Awawa*, or Agaws bordering on Little Damot."† This, again, is my *Aghaghá*, or Agau of Agaumider, of which a vocabulary is also given by Bruce. (XVIII. 6, 7.)

3. The Ḥwarasa, "spoken in Kwara or Ḥwara, and by the *Falosha* (Falocha) of Gallagar, Kayla, and of the Awawa;" which is the *Falásha* of Bruce and myself. (XVIII. 4, 5.)

M. d'Abbadie adds, that "short vocabularies show that the Agau languages are closely allied to the Gabi, spoken by the Bileu (probably the Blemmyi of the Romans), and to the languages of Atala in Simen [Sạ́mien], of Alafa and of Kwara, or Ḥwara;" and he alludes also to the "*kindred* dialect of the Gimant [Kamáunts]." All this is in conformity with my views as to the aboriginal character of the Agau class of languages, and of their great extension throughout Abessinia,* which has already been adverted to under No. XVII.

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* No. 911, pp. 359, 360.

† By "Little Damot," M. d'Abbadie means the "Damot" of the maps.

B. The Galla Class, which consists of—

1. The Afar Proper, "spoken by the *Adál*, Taltal, Talfen, &c.;" which is no other than the *Adál*, or Dankáli (*plur.* Danákil) of modern travellers in Abessinia (Latham, XIX. B. 1, 2, 5, 6, 7, 8).

2. The *Saho*, "spoken by the Hazaorta and Toroua," is, in like manner, the language of the Shihos, or *Shohos*, who dwell between Massówah and the high land of Tigre (XIX. B. 3, 4).

3. The Ilmorma, spoken "by the Orme or Oromo, *better known under the name of Galla*." Of course, this is the widely-spread *Galla* tongue, with which we have now become so well acquainted, through the labours of Krapf and Tutschek (XIX. A.).

4. "The Szomaliod, spoken by the *Szomal*," which again is merely the language of the equally well known Somaulis or Somális (XIX. C.), of which name the Arabic plural is *Somál.*

The Tufte (Toufte) is stated by M. d'Abbadie to be "spoken by a small nation near the Tambáro, and issued, according to their own traditions, from the same ancestors as the Orme," *i. e.* the Gallas. Of this language, the traveller's collection consists of *ten* words, which, as he himself observes, "is only better than nothing at all." I would add, that such a number of words is scarcely sufficient to enable us to class this language, which, even in spite of the tradition alluded to, I am inclined to place among those of the *Gonga* class (XX.), by which it is geographically surrounded.

My opinion is the same with respect to the neighbouring language of Tambáro, which M. d'Abbadie considers to be "a member of the Amhára family" (XVII.), but which I would equally place in the Gonga class (XX.).

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* These opinions were first expressed by me in *A Statement of Facts relative to the Transactions between the Writer and the late British Political Mission to the Court of Shoa,* p. 13, *n.*

In support of this opinion as to both these languages of Tufte and Tambáro, I may cite the authority of Dr Krapf, as repeated by Major Harris,* that the language of Kaffa "is common to Gobo, *Tufftee*, and *Dumbaro*." This latter mode of spelling the name, which, correctly pronounced, is *T*'ambáro or *Ts*ambáro, arises from the habit so common among the natives of Upper Germany, of confounding the sounds of the hard and soft consonants.

C. THE GONGA GROUP, styled by M. d'Abbadie "the *Chamitic* languages of Great Damot." These consist of—

1. The Sidama.
2. The Daurua (Dawrooa).
3. The Yạmma or Yángara.
4. The Shay.

Dr Latham has already adverted to the fact that the first three of these languages are identical with those of Kaffa, Worạ́tta, and Yángaro, of which vocabularies have been given by me in the Proceedings of the Philological Society. Sidáma is the name by which Kaffa is known to the Gallas ;† Dáwaro (Dawrua) is either the same with, or a part of, the country of Worạ́tta ;‡ and Yạmma is, as M. d'Abbadie himself shows, the same as Yángara, that is to say, my Yángaro or Djándjạro, which is the Gingiro of the maps. As to that traveller's Shay, which he describes as "a language spoken by the Gimira, Gamaru (Gamarou), or Gamru," and which, he says, "a collection of 400 words induces him to place side by side with the Sidama," this tongue seems, from another statement of the same traveller, to be merely a *dialect* of the language of Kaffa, if it be not absolutely identical with it. In speaking of the country of Kaffa, he says, "Kaffa est le nom des Gallas, les Abyssins disent Sidama, *et les indigènes appellent leur pays Gomara.*"§ In this statement there is, however, an error : it is the Abessinians who say Kaffa, and the Gallas who say Sidáma.

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* *Highlands of Æthiopia*, vol. iii. p. 6.

† *Journal of the Royal Geographical Society*, vol. xiii. p. 261.

‡ *Ibid.*, map.

§ *Bulletin de la Société de Géographie de Paris*, 2d Series, vol. xviii. p. 355.

M. d'Abbadie further mentions the Nao language as appearing to be "a mere dialect of the Shay," and that of Hadiya-Wanbe as being "in close contact with the Dawrua tongue." I do not possess the means of determining the precise locality in which the former of these dialects is spoken, but it is manifestly in the immediate vicinity of Kaffa, if, indeed, it does not form a portion of that country. Hadiya is the *Hadea* of the maps, and it lies to the south-west of Gurágie,* and to the north-east of the other countries in which the cognate languages of the Gonga class are spoken.

M. d'Abbadie remarks, that "the Gonga language spoken on both sides of the Abai, is closely allied to the Sidáma." This has already been demonstrated by my published lists,† which comprise a copious vocabulary of the Gonga tongue of Shínasha, a district situate in the valley of the Abai, to the south of Damot;‡ and it is on account of the affinity which all the languages comprised in this class have to one another, that I have attributed to them the generic denomination of *Gonga*. The irruption of the Gallas has much contracted the limits within which the languages of this class are spoken. Ludolf informs us,§ that the Gonga tongue was formerly that of Enárea, and he cites one word, *donzo*, as meaning "lord," or "master," which corresponds with the *dondjo* (Gonga of Shínasha) and *dono* (Kaffa) of my vocabularies.

D. THE BIDJA LANGUAGE spoken at Suákin, with respect to which it is sufficient to refer to the remarks already made on Dr Latham's Class, XVI.

E. SEVERAL NEGRO LANGUAGES, of which M. d'Abbadie says he has collected trifling vocabularies, remain "unplaced" by Dr Latham. They are as follows:—

1. Gwinza.
2. Suro (Souro).
3. Doko (Dokko).
4. Yambo.
5. Gamo.
6. Bárea.

To which has to be added, (7.) The Kónfal.

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* See *Journ. Roy. Geogr. Soc.*, vol. xvii. p. 60, *n*.

† *Proceedings of the Philological Society*, vol. ii. pp. 97–107.

‡ *Journ. Roy. Geogr. Soc.*, vol. xiv. p. 39.

§ *Hist. Æthiop.*, lib. i. cap. 15.

Respecting the Gwinza and Gamo, I am not able to say anything. But the others may be thus classed:—

The Suro and Doko are two dark-coloured if not absolutely negro people, dwelling in the vicinity of Kaffa.* Of the country of Suro, I have already said, in another place,† that it "is two days' journey to the west of Bonga, and is subject to Kaffa. The country is both highland and valley, but the people are all *Shánkạlas* or negroes. The men go naked, and the women wear only a small apron. The king of the country alone is clothed. They are pagans. They take out two of the lower front teeth, and cut a hole in the lower lip, into which they insert a wooden plug. They also pierce the gristle of the ear for the insertion of grass."‡ It can scarcely be

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* *Journ. Roy. Geogr. Soc.*, vol. xiii. pp. 264, 265. † *Ibid.*

‡ The country of these Suro negroes was, at the same time, described by 'Omar ibn Nedját, the intelligent Abessinian merchant from whom I obtained the above information respecting them, as lying in the valley of the river Gódjeb, at a short distance to the west of Kaffa. (See his map, in *Journ. Roy. Geogr. Soc.*, vol. xvii. part i.).

M. Ferdinand Werne, who accompanied the second Egyptian expedition up the White River, has recently published an account of his voyage (*Expedition zur Entdeckung der Quellen des Weissen Nil*, Berlin, 1848), in various parts of which similar customs are described as prevailing among the black inhabitants of the valley of that river. The traveller states, that, as far south as Bari, a country in the fourth parallel of north latitude, all the natives are in the habit of extracting several of the incisors, both of the upper and of the lower jaw, "in order that they may not resemble beasts of prey" (p. 188); and that they also "pierce the cartilage of the ear all round, and, in the absence of beads or other ornaments, they insert in the orifices small pieces of wood" (p. 428). The natives of Bari alone form an exception, being "distinguished" (says M. Werne) "from all the people we have hitherto seen, by the circumstance, that they do not pierce the ears for the insertion of ornaments; and also, that they are not tattooed" (p. 293); and higher up the river than Bari, which country was the extreme point reached by the expedition, the natives are said to "keep in all their teeth" (p. 325).

From a comparison of these particulars, the conclusion may fairly be drawn, that the Suro negroes are of the same race as the inhabitants of the valley of the White River *below Bari*, but not as those *above* that country; and as they occupy the valley of the Gódjeb, which is now known to be an affluent of the Nile; and as there is no important stream joining the White River from the east *below Bari*, except the Sobát, Télfi, or River of Habesh; it results that this latter river is the lower course of the Gódjeb. This conclusion is, of course, quite independent of all other arguments already adduced by me in support of the same position. See *Journal of the Royal Geographical Society of London*, doubted that they are of cognate origin with the negro inhabitants of the valley of the White River, and that, consequently, their language belongs to the Nubian class (XIV.)

As regards the Dokos, I believe I was the first, in 1841, to make public mention of these people,* from information obtained by Dr Krapf and myself from a slave of the king of Shoa, named Dilbo; but the particulars then furnished were far from going to the extent of those which have since been given by Dr Krapf, and after him by Major Harris. Ethnologists will remember the marvellous stories related respecting these Dokos, who are described as a nation of pigmies, of scarcely human character, "not taller than boys nine or ten years of age, and never exceeding that height, even in the most advanced age," and who are said to be employed as domestic servants by the people of Kaffa. It is sufficient to refer to Dr Prichard's *Natural History of Man*,† for Dr Krapf's Report on this subject, which was originally published in the *Monatsberichte* of the Geographical Society of Berlin.‡

I must here repeat the expression of the doubts which I entertained from the outset,§ on many of the points thus related by Dilbo. When questioned by Dr Krapf and myself, his statements were such as to entitle him to full credit; and I am afraid that he was subsequently induced to enter into these fanciful and exaggerated details, by a feeling not uncommon among uneducated persons, when *pressed* to furnish information, that the more wonderful they make their story, the greater praise they will obtain; and probably, also, the greater reward.

The description given by M. d'Abbadie of the Dokos, is remarkably at variance with that furnished by Dilbo to Dr Krapf. The former traveller says,‖—"My Sidama interpreter was a Dokko, freed by his master's death. This man remained nearly two years with me, and was eighteen centimètres

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vol. xvii. p. 44, *et seq.*; *Bulletin de la Société de Géographie de Paris*, 3d Series, vol. viii. p. 356, *et seq.*; *Edinburgh New Philosophical Journal*, vol. xlv. p. 238, *et seq.*—22d November 1848.

* See *Friend of Africa* (October 1841), vol. i. p. 187; *Journ. Roy. Geogr. Soc.*, vol. xii. p. 87; vol. xiii. p. 265, *et seq.*

† 2d Edit., p. 553, *et seq.*

‡ Vol. iv. p. 181, *et seq.*

§ See *Literary Gazette* of Dec. 30, 1843, No. 1406.

‖ *Athenæum*, of March 8, 1845, No. 906, p. 243.

[seven English inches] shorter than myself. I have seen three other Dokko, all black-like negroes, but with a fine facial angle like the Mozambique natives, and rather small, —what we call *trapu* in France, but nothing like pigmies. . . . Their forms are the most perfect *mezzo-termine* between Ethiopians and negroes. They use, according to their own account, the *Sorghum vulgare* to make bread, and have a name (elmos), for bread." In another place,* the same traveller describes the Dokos as being " *très gros* et bien musclés, absolument comme les Sawahily."

It is necessary to explain, that the word *Doko* is not to be regarded as the proper name of any particular people. In the Galla language it is an appellative, signifying " ignorant," " stupid;" and it appears to be used in the same indefinite sense as our expression " savage."

The evident mixing up of *monkeys* in the description given of these Dokos or savages, may be accounted for in the same way, probably, as a statement made in Sir Gardner Wilkinson's *Manners and Customs of the Ancient Egyptians*,† respecting the employment of those animals as domestic servants in Djimma, a country to the south of Abessinia, situated between Enárea and Kaffa. Djimma is also adjoining to Yángaro, which latter country, as I have already mentioned, is called Djándjạro by the Gallas. In the wars which are continually taking place between Djimma and Yángaro, many natives of the latter country are made prisoners, some of whom are retained in domestic slavery, while others are sold into captivity. Hence, the number of Yángaro or Djándjạro slaves that are met with in the markets of Abessinia and the Red Sea. But in Abessinia, the name Djándjạro has, partly through ignorance and partly by way of ridicule, been changed into *Zéndjero*, which word in the Amharic language means " Monkey." If, then, in the enunciation of what is unquestionably a fact, it were stated that the people of Djimma and Kaffa are waited on by Djándjạro servants, or, as it would be repeated in Amharic, by *Zéndjero* servants; nothing would be more natural among an ignorant and marvel-loving people, than to imagine these " Zéndjero" to be *monkey* servants.

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* *Bulletin*, 2d Series, vol. xix. p. 439.

† Vol. ii. p. 151.

I was once gravely informed by an Abessinian slave-merchant of the market of Baso in Godjam, that beyond Kaffa there is a country, the male inhabitants of which are all dogs, and the females are women; and that the dogs go out to tend the cattle, while the women occupy themselves with domestic affairs. It was of little avail to inquire how it came to pass that the progeny of this strange union should be canine on the male side, and human on the female. *That* my informant did not know: the other he did know; though he honestly admitted that he had not been so far as to have seen it himself. This story I consider to have originated in the fact, that beyond Kaffa there is truly a "Dog" country, just as, adjoining to Djimma there is a "Monkey" country; that is to say, in Worạ́tta there is a place or district named *Ushá*,* which word in Amharic means "dog," in the same way that *Zéndjero* means "monkey." As is usual in such cases, the story was afterwards invented to account for the name.†

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* See my map in *Journ. Roy. Geogr. Soc.*, vol. xiii.

† According to M. Werne (*Expedition zur Entdeckung der Quellen des Weissen Nil*, p. 325), a fable prevails among the natives of the valley of the White River, respecting a race of cannibals, *having heads like dogs and going on all-fours*, who are said to inhabit the mountains of Logayà, to the east of Bari. The following reasonable explanation of this monstrous story was, however, given to that traveller by Lákono, the intelligent king of the giant race of Bari. He stated that, "in reality, these wicked people have heads like those of other human beings; only *they keep in all their teeth*. [It has been remarked (p. 273, *note*) that the negro inhabitants of the valley of the Nile extract the incisors, in order that they may not resemble wild beasts]; and when they come to eat up others, *they creep in on all-fours*." On this M. Werne himself remarks, that "most likely the simple meaning of this is, that these alleged cannibals do not engage in open war with their neighbours, but sneak in among them like dogs, and carry away individuals, whom *perhaps* they may devour."

For myself, I question much the existence of cannibalism among these mountaineers, whom the mere fact of their not extracting their teeth proves to be of less barbarous habits than the natives of the low country.

From the earliest times cannibalism has been said to prevail among the inhabitants of Africa. It is only necessary to allude to the Αἰθίοπες ἀνθρωποφάγοι of Ptolemy, and to the *Nyam-Nyam*, *Lem-Lem*, *Dum-a-Dum*, &c., of the Arabian geographers and of the modern Arabs. But stories of this kind require indisputable evidence to establish their truth; and there is no doubt that they often originate in ignorance, if not in interested motives, on the part of their promulgators.

Of the language of these Dokos M. d'Abbadie states that he possesses only *twenty-nine* words; which is rather surprising, when we consider that he says he had one of these people by him as an interpreter for nearly two years. Its affinity with the language of Worạ́tta is, however, asserted by that traveller.* This will place it in the Gonga class, which is quite in accordance with its geographical position. The only word of the language cited by M. d'Abbadie is *elmos*, signifying "bread;" which expression, however, I cannot connect with any word in my vocabularies. Dr Krapf states,† that "the language of the Dokos is a kind of murmuring, which is understood by no one but themselves and their hunters." But he also mentions, that

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Mr Robertson, in his *Notes on Africa* (London, 1819), pp. 353–354, when treating of the natives of the south bank of the river Congo, says: "The opinion that these, or any other people on this [the west] coast, are Anthropophagi, is ridiculous. One of the traders at Bonny did, indeed, once tell me so plausible a story concerning the Quas eating those who fell into their power, that I was half inclined to believe him. But subsequent inquiry convinced me that there was no truth whatever in it. . . . The Portuguese having taken so much trouble to impress other nations with the dreadful effects of man-eating, *they, of course, consider this country of some value*; but from their not having been devoured themselves, it seems other animal food is plentiful; or, perhaps, the aboriginal inhabitants think the Portuguese rather coarse meat, and like cattle or fish better."

A similar tale of cannibalism is related in Shoa, and for a somewhat similar reason. The wilderness of Gédem, a province in the north of that kingdom, which I visited in April 1841, is "the place of refuge for all the fugitives and bad characters of Shoa" (*Journ. Roy. Geogr. Soc.*, vol. xii. p. 92). The Dankáli slave-merchants trading between Shoa and the coast take care to impress on the minds of their slaves that the people of Gédem are cannibals, *who will be sure to eat them up if they run away*. Hence, the poor children are content to remain with their (often cruel) masters, rather than run the chance of being devoured by the wicked man-eaters of Gédem!

To shew the little value, generally, of these stories of the existence of cannibalism among unknown nations, I may cite, further, the following passage from the interesting *Notes sur les Mœurs, Coutumes et Traditions des Amakoua* of M. Eugène de Froberville, published in the *Bulletin de la Société de Géographie*, 3d Ser., vol. viii. p. 324:—"Les traditions les plus effrayantes sont répandues par toute l'Afrique orientale touchant le sort des esclaves transportés dans le pays des blancs. Mes informateurs m'ont unanimement déclaré que l'on croyait généralement que *les blancs mangeaient les esclaves qu'ils viennent chercher en Afrique*."—Mutato nomine, de te fabula narratur!—22d November 1848.

* *Bulletin*, 2d Ser., vol. xix. p. 439.

† Prichard, *Natural History of Man*, p. 556.

they address the Supreme Being by crying, "Yer! Yer!" Now *Yero*, in the Kaffa language, means "God;" so that we have here a further proof that the language of these Dokos belongs to the Gonga class.

In commenting, on a former occasion, on a few words given by Dr Krapf, apparently from the mouth of Dilbo, as specimens of the language of Kaffa, I remarked,* that "they do not altogether agree with *my* Kaffa words, which I obtained from persons who were most assuredly natives of Bonga, the capital of that country. From Dilbo's physical appearance and other circumstances, I have reason to believe that he was a native, not of Kaffa itself, but of some neighbouring country, which will account for the difference of language." I may now add, that the description given by M. d'Abbadie of the physical character of the Dokos corresponds so closely with that of Dilbo, that I am inclined to think he was himself one of those savages. Should this really be the case, the words inserted between brackets in my Kaffa vocabulary will most probably belong to some Doko dialect.

The next of M. d'Abbadie's unplaced languages is that of *Yambo*, which is the name given by that traveller to the negro inhabitants of certain islands formed by the Baḥr el Abyaḍ, or the direct stream of the Nile. These islands are placed by him as high up the stream as about 6° N. lat.; but, as has been shown by me in a communication recently made to the Geographical Society of Paris,† their true position is in about 9° N. lat., below the confluence of the Sobát or River of Habesh. Thus, these Yambos appear to be Denka negroes, and their language will consequently belong to the Nubian class (XIV.).

The *Bárea* is said by M. d'Abbadie to be "spoken by the semi-negroes near the Tạkkạzie;" that is to say, the "Shánkạlas" of that river; so that this language is identical with either the Bárea of Salt (XXIII.), or with the Dalla (XXII.)

And lastly, we have the *Kónfạl*, who are stated to "live between Kwara and the Awawa"—that is to say, the Aghaghá or Agaus of Agaumider. Of this language M. d'Abbadie says, that he has "no sample beyond the first ten numbers, which are partly Gĭís;" and he adds that "the all-but-unknown Kónfạl tribes are the most perfect medium between the straight-nosed Ethiopian and the grovelling negro." But the position thus attributed to the Kónfạl corresponds precisely with that of the black inhabitants of Gíndjar already mentioned; and when M. d'Abbadie asserts that their numerals are partly Geez, *i. e.*, Ethiopic, he should rather have said *Arabic;* since the fact is beyond dispute that the language of Gíndjar is, in great part, a corrupt Arabic, and it is not less a fact that the Ethiopic and Arabic numerals are almost identical.

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* *Proceedings of the Philological Society*, vol. ii. p. 94, *n.*

† *Bulletin*, 3d Ser., vol. viii. p. 356, *et seq.*

The conclusion to be drawn from this investigation is, that Dr Latham's list, whatever modifications increased information may eventually give occasion to introduce into its arrangement, is, in fact, exhaustive of the languages of Abessinia and the countries immediately adjoining.

The map in which the results thus arrived at have been embodied, makes no pretensions to strict accuracy in the limits that are assigned in it to the several classes of languages. Our materials are still too imperfect to admit of precision in this respect. The only merit that this map can lay claim to, is that of affording a general *coup-d'œil* of the geographical distribution of the various languages spoken in that portion of Africa which has more immediately fallen within my cognizance; and thus of obviating, so far as Abessinia and the neighbouring countries are concerned, the difficulty which, as the author of the Report justly complains, so frequently arises from the absence of any geographical notice respecting the districts within which a particular language is spoken.

London, 31*st May* 1848.

Instructions for Collecting and Preserving Invertebrate Animals. By RICHARD OWEN, F.R.S., Hunterian Professor to the Royal College of Surgeons of England.

As water is the element in which the greater number of the classes of animals exist, and as the sea is the scene of such existence, and the field of research which will be most commonly presented to those for whom the following instructions for collecting and preserving animals have been drawn up, they will commence with the marine species, and the lowest forms of animal life.

Algæ, Sponges, Corallines, and Corals.

The line of demarcation between the vegetable and animal kingdoms is so obscurely marked in the lowly organised marine species, and the modes of collecting and preserving these are so similar, that the kindred groups above named are associated together as the subjects of the following remarks.

Algæ, commonly called sea-weeds, may be divided, for the convenience of the collector, into three kinds, according to their colour:—

1. Olive-coloured (*Fuci*), generally of large size and leathery texture, rarely gelatinous; usually laminate or leafy, rarely filamentous or thready.
2. Red-coloured (*Florideæ*), firm, fleshy, or gelatinous; usually filamentous, sometimes membranaceous.
3. Green (*Chlorosperms*), membranaceous or filamentous; rarely horny.

Sponges are bodies usually adherent in irregular or amorphous masses, rarely in the form of hollow reticulate cones; composed of a soft, jelly-like tissue, supported by siliceous or calcareous spiculæ, or by horny filaments. They are divided, accordingly, into horny, or "keratose," siliceous and "calcareous" sponges. Their soft, organic substance is commonly diffluent, and drops from the firmer basis, when removed from the water, or it is easily washed away. It exhibits no signs of sensibility; no contraction or retraction when touched or otherwise stimulated. The evidence of life is afforded, as in the corallines and algæ, by the flow of currents of water through canals, entering by pores, and in the sponges escaping by larger orifices; and an appearance of animal life is given to both algæ and sponges by the locomotion of the sporules or gemmules.

Corallines are plants coated with a calcareous covering, either red or green when fresh, becoming white and brittle on exposure to the air.

Corals, though called "zoophytes," are true animals; the currents which permeate them enter by "mouths," always provided with a crown of feelers or seizers, called tentacles, and communicating with digestive sacs or "stomachs," into which the pores of the nutrient canals open. The tentaculated mouths are called "polypes." Their fleshy tissue, as well as that which connects them together into an organic whole, when the coral is compound, or has more than one mouth, is "sensitive," or retracts and shrinks when touched. For the purposes of the collector, corals may be divided into the "fleshy" (*Polypi carnosi*), in which the flesh has no firm supporting part; the "horny or flexible," usually having this supporting substance as an external tube; and the "calcareous," in which the supporting substance is usually covered by the animal matter or flesh, forming an internal skeleton, usually of one piece, rarely jointed.

The above-defined classes of organised beings, which all present the "habit," or outward form, more or less, of plants, are found from the extreme high water mark to a depth of from 50 to 100 fathoms. Living algæ rarely descend below 50 fathoms, but corals of the genera *Lepralia*, *Retepora*, and *Hornera*, have been dredged up from 270 fathoms, and fragments of dead coral from 400 fathoms.* Specimens within the reach of the tide are to be collected at low water, especially of spring tides; the most interesting species occur at the verge of low water mark. Those that dwell at greater depth must be sought by dredging, or by dragging after a boat an iron cross furnished with numerous strong hooks. One or more strong glass bottles with wide mouths, or a hand-basket lined with japanned tin, should be provided for the purpose of bringing on board the smaller and more delicate species in sea-water, and they should be kept in it, the "Florideæ" more especially, until they can be arranged for drying, or other modes of permanent preservation can be attended to.

In collecting algæ, corallines, or the branched, horny, or calcareous corals, care should be taken to bring the *entire specimen, with its base or root.* With respect to the coarser algæ, it is merely requisite, for the purpose of transmission, to spread the specimens immediately on being brought fresh from the sea, without previous washing, in an airy situation to dry, but not to expose them to too powerful a sun; if turned over a few times, they will dry very rapidly. When thoroughly dried, they may be packed loosely in paper bags or boxes, and will require only to be remoistened and properly pressed, in order to make cabinet specimens. For the purpose of transmission, it is better *not* to wash the specimens in fresh water previous to drying, as the salt they contain tends both to preserve them and to keep them pliable, and more ready to imbibe water on reimmersion. With respect to the delicate algæ,—"The collector should have two or three flat dishes, one of which is to be filled with salt water and two with fresh; in the first of these the specimens are to be rinsed and pruned, to get rid of any dirt or parasites, or other extraneous matter; they are then to be floated in one of the dishes of fresh water for a few minutes, care being taken not to leave them too long in this medium, and then one by one removed to the third dish, and a piece of white paper, of the size suited to that of each specimen, is to be introduced underneath it. The paper is to be carefully brought to the surface of the water, the specimen remaining displayed upón it, with the help of a pair of forceps or a porcupine's quill, or any fine-pointed instrument; and it is then to be gently drawn out of the water, keeping the specimen displayed. These wet papers, with their specimens, are then placed between sheets of soft soaking-paper, and put under pressure, and in most cases the specimen adheres in drying to the paper on which it is laid out. Care must be taken to prevent the bloating-paper sticking to the specimens, and destroying them. Frequent changes of drying-paper (once in six hours), and cotton rags laid over the specimens, are the best preservatives. The collector should have at hand four or five dozen pieces of unglazed thin calico (such as sells for 2d. or 3d. per yard), each piece about eighteen inches long and twelve inches wide, one of which, with two or three sheets of paper, should be laid over every sheet of specimens as it is put in the press. These cloths are only required in the first two or three changes of drying-papers; for, once the specimen has begun to dry, it will adhere to the paper on which it has been floated in preference to the blotting-paper laid over it."*

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* Capt. Sir J. Ross's "Antarctic Voyage," Appendix, No. IV.

For dried specimens of corallines, corals, and sponges, it is advisable to soak the specimen for a time in fresh water before drying. They may then be packed among the rough-dried sea-weeds, in boxes; but the more delicate specimens should be placed in separate chip-boxes with cotton.

With regard to corals, &c., it must be remembered that dried specimens are but the skeletons of those animals, and that only the "horny" and "calcareous" species can be so preserved. The "fleshy" kinds, commonly known as "polypes," "sea-anemones," or "animal-flowers," must be preserved entire in alcohol or saline solution, and of the latter the following (No. 1 of Goadby's recipes) has been found successful:—

Solution, No. 1.

Bay salt, ...	4 oz.
Alum, ...	2 oz.
Corrosive sublimate, . . .	2 grains.
Rain water, ...	1 quart.

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* *Dr Harvey*, in Mr Ball's Report on the Dublin University Magazine, p. 3.

In order to preserve the specimens expanded, they should be removed and placed alive in a dish of sea-water; and when they have protruded and expanded their tentacles, the solution should be slowly and quietly added to the sea-water, when the animal may be killed and fixed in its expanded state. So prepared, the specimens should be transferred to a bottle of fresh solution.

In like manner the minute polypes of the flexible or horny corals may be preserved, protruded from their cells, and expanded. If a small piece of corrosive sublimate is put into the vessel of sea-water containing such living polypes, it will kill or paralyse them when protruded, as it slowly dissolves; but they must be removed as soon as they have lost their power of retraction, otherwise their tissue is rendered fragile, or is decomposed. The polypes, or animal part of the calcareous kinds, called "Madrepores," "Millepores," "Fungæ," "Red Coral," "Gorgoniæ," &c., require for their preservation, in connection with their supporting basis, the following solution (No. 2.):—

Solution, No. 2.

Bay Salt, ...	½ lb.
Arsenious acid, or white oxide of arsenic,	20 grains.
Corrosive sublimate, . .	2 grains.
Boiling rain-water, . . .	1 quart.

All the polypes concerned in the formation of coral reefs, atolls, or coral islands, may be preserved in the above solution, provided they be killed by its gradual application as above described, and be afterwards transferred into fresh solution. With regard to the structure and formation, and mode of observation of coral islands and reefs, the work by Charles Darwin, Esq., *on the Structure and Distribution of Coral Reefs* (8vo, 1842), should be consulted.* Never fail to ascertain, if possible, to what depth below the surface of the sea the corals descend, and on what basis they rest; and for particular instructions with reference to coral reefs, see Mr Darwin's remarks under the head of "Geology."

Infusorial Animalcules (Polygastria, Polythalmia, Phytolitharia.)

Some idea of the value and importance of attending to the collection of these microscopical organized beings may be had by reference to Ehrenberg's observations, forming Appendix, No. V. of *Captain Sir James C. Ross's Antarctic Voyage*, vol. i., p. 339; a better idea by the perusal of Ehrenberg's numerous communications to scientific journals, some of which have been translated in *Taylor's*

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* See also, on this subject, Lieutenant Nelson's paper "On the Geology of the Bermudas;" Geological Transactions, Second Series, vol. v., pp. 103–123.

Annals of Science; and the best idea by the study of Ehrenberg's great work, *Entwickelung Lebensdauer und Struktur der Magenthiere und Räderthiere*, &c., folio, 1832. The important relations of these minutest forms of animal life to great questions in geology, to the alteration of coast-lines, and to the phenomena of oceanic luminosity, make it indispensable to include them in directions for collecting facts in natural history.

Whenever the surface of the sea presents a difference of colour and density, in the form of pellicles, streaks, or shining oil-like spots, lift up portions by dipping in thin plates of mica, or stout paper, and raising them horizontally; dry these and preserve them in a book, noting the latitude and longitude, the time of day, and the temperature of the sea. The animalcules remain attached to the pieces of paper or mica employed in their capture, and may be determined by subsequent microscopical observation.

Where the sea seems pure and colourless, a bucketful may be raised and strained through fine linen; by repeating this act, a portion will commonly remain on the filter, which is then generally rich in invisible animalcules, and should be preserved in small glass bottles or tubes, with a buble of air between the cork or stopper, and the water. Any visible gelatinous acalephæ should be removed and placed in spirit of wine, or the solution No. 1. Specimens of sea-water thus saturated with animalcules should be prepared at each degree of latitude and longitude traversed on the voyage, by which means the geographical distribution of these minute organisms may be ascertained, when the species so collected are determined, after the voyage, by microscopic observation.

Small bottles or tubes of the water of each mineral spring or hot spring should be preserved for the same purpose. In a deposit from melted pancake-ice from the Barrier, in 78° 10′ S. lat., 162° W. long., brought home in Ross's antarctic voyage, Ehrenberg detected of siliceous-shelled polygastria fifty-one species, including four new genera; siliceous phytolitharia, twenty-four species; and of calcareous-shelled polythalmia, four species. Small packets of the sand of each coast that may be visited, and of the sand or mud brought up with the anchor or the sounding-line, should be preserved; the localities or latitude and longitude being precisely noted in each case.

Acalephæ (Sea-blubber or Medusæ, Portuguese Men-of-war, Jelly-Fish, and other floating Marine Gelatinous Animals.)

The brilliant but evanescent hues of many of this class of animals can only be preserved by coloured drawings executed at the time of capture. The solution, No. 1, will suffice for the preservation of the animals themselves, provided it be changed after they have remained in it about twenty-four hours, for most of the gelatinous animals, especially the Medusæ, contain a great quantity of fluid, which, mixing with the preserving liquid, dilutes it, and renders it unfit for long-continued preservation. The best preserved specimens of these delicate animals are those that have been placed immediately after capture in the solution No. 1, diluted with an additional pint of rain-water, and which have been afterwards transferred to fresh solution of the proper strength. Glass-stoppered bottles with wide mouths are the best adapted for the larger Acalephæ.

Echinoderms (Star-Fish [Asterias], Sea-Urchins [Echinidæ], Trepang or Sea-Cucumbers [Holothuriæ]).

For the preservation of the entire animal, with the soft parts of a Star-fish (*Asterias*), or a Sea-urchin (*Echinus*), the arsenical solution (No. 2) is preferable; the softer Trepangs (*Holothuriæ*) may be preserved in either solution. It should be gradually added to the vessel of water in which the living specimen is at rest, in order to kill it, with the soft appendages protruded or elongated. This is particularly requisite in the case of the *Holothuriæ*, which, if plunged suddenly in solution, are apt to squeeze out and rupture their viscera. With regard, however, to long and slender Star-fishes (Ophiuræ), sometimes called "brittle stars," from their habit of breaking themselves into pieces when captured, these should be instantly plunged into a large basin of cold fresh water, when they die in a state of expansion, and too quickly for the acts of contraction by which the rays are broken off. After lying for an hour or so in the fresh water, they may be transferred to the solution; if preserved dry, they should be dipped for a moment in boiling water, then dried in the sun or in a current of air, and packed in paper. When the specimens have soaked in solution one or two days, according to the temperature, they should be removed into fresh solution. The Echini should be sewed up each in a separate bag of muslin, and not be crowded so as to press upon each other in the same bottle. The Star-fish and Sea-urchins that are preserved dry should be emptied of their viscera or soft contents by the mouth or larger (lower) aperture, and should then be soaked in fresh water, changed two or three times, for so many hours, or until the saline particles of their native element have been extracted, before they are dried. The Echini should be wrapped up in cotton, and sewed up, each in its separate bag, in order to preserve the spines, which may become detached in the course of a voyage, and are apt to become so if the precaution of soaking away the saline particles be not previously taken. All Echini and Star-fish should be examined for small shells (*Stylifer* of Broderip, for example), which nestle in and among the rays, and at the roots of the spines, and for other parasites.

Recent Pentacrini (Lily-stars), especially their bases, will be valuable acquisitions. They may be dredged up of large size in tropical seas; as those of Guadaloupe, for example.

Entozoa (*Intestinal Worms and other internal parasites*).

These are to be preserved either in solution No. 1, or in colourless proof-spirit. This class of animals has been too much neglected by collectors. Every animal that is opened and dissected, especially fishes, may present rare or undescribed species of Entozoa. The eyes of fishes are often the seat of such; the noses of sharks are frequently infested by them. They may be found not only in the alimentary canal, but in the tissues of most of the organs. When the parasite is adherent, the part to which it adheres should be removed with it, care being taken to secure the whole mouth or proboscis of the parasite. When it is encysted in an organ, the cyst is to be removed entire with the surrounding tissue of the organ. Portions of muscle or other tissue which appear speckled with minute white spots should be preserved, as these may be occasioned by the cyst of *Trichinæ*, or allied microscopic Entozoa. The number attached to the specimen should correspond with that in the list, having reference to the animal, and part or organ infested by the parasite.

Epizoa (*Lerneæ or Fish-lice, and other external parasites*): *Annelides* (*Leeches, Worms, Nereids, or Sea-centipedes, Tube-worms, &c.*).

The exterior surface, the mouth, and the gills of all fishes, should be examined for parasitic animals, some of which exhibit the most extraordinary forms and combinations of structure, as, *e. g.*, the *Diplozoon* of Nordmann, a genus of Entozoa, from the gills of the bream. When the parasites adhere firmly to the part, they should be cut out with the adhering organ entire, which sometimes penetrates to a great depth in the flesh. The exterior surface of porpoises, grampuses, and the larger species of the whale tribe, should be scrutinised for adherent parasitic animals. Rare kinds of leeches may be found on fishes, as, for example, the *Bramhellion* of the torpedo. A species of leech, with external tufted gills, *Hirudo branchiata*, has been detected on a marine tortoise or turtle in the Pacific, the anatomical examination of which is especially recommended by Cuvier. Leeches, and all the various kinds of sea-worms, comprehended under the class-name "Annelides," and including the Nereids, or Sea-centipedes, usually found amongst sea-weed or under stones, sometimes attaining the length of twelve feet;* and the tube-worms usually crowned with brilliant coloured tentacles, may be preserved in the solution No. I., or in colourless spirit. Those, however, as the *Serpulidæ*, that form calcareous tubes, should be preserved in the solution No. II. In all cases it is desirable that the specimens should be allowed to die gradually in the water they inhabit, when they commonly display their natural external form and appendages in a relaxed state ; they should then be immediately put into the solution or spirit, to prevent putrefaction, which otherwise takes place rapidly.

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* See the specimen, from Bermuda, of *Leodice Gigantea*, No. 253, A. Museum, College of Surgeons, London.

Cirripedia (Barnacles and Acorn-shells, or Crown-shells).

The barnacles, or pedunculated Cirripedes, with soft stalks, should be preserved in the solution No. II., or in spirits; they are commonly attached to floating timber, and the smaller species to seaweed, shells, &c. The sessile kinds (acorn-shells, &c.), which encrust the coast rocks all over the world, and are found parasitic on turtles, whales, &c., should likewise be preserved in spirit, or solution No. II., as the included animal is necessary in some genera for the recognition of the species. The colours of the pedunculated should be noted whilst fresh. If the sessile kinds are preserved dry, the included animal ought never to be taken out. In removing all the kinds from their points of attachment care must be taken that in some specimens, at least, the base, which is either membranous or calcareous, be preserved. It is particularly desirable that some young, as well as large specimens, should be collected. In the tropical seas certain corals and shells contain embedded in them singular forms of cirripedes, which, presenting externally little more than a single aperture, are easily overlooked; such kinds had better be preserved in the coral. Others live embedded in sponges; two genera live on whale's skin (*Coronula* and *Tubicinella*), the development of which needs to be studied by specimens of the ova and young; another less-known genus (*Chelonobia*) lives partly embedded in the skin of turtles; a third attaches itself to the manatee or sea-cow; and some small and interesting species of barnacle are parasitic on sea-snakes. Lobsters, crabs, bivalve and other shells, as well as floating pieces of wood, or even net corks, become the habitat of animals of the class *Cirripedia*. It should always be noted to what animals these parasitic Cirripedes are attached, as well as any circumstances that may determine the period during which they have remained attached.

Crustacea (Shrimps, Sea-mantises, Cray-fish, Lobsters, Crabs, and King-crabs).

All the animals of this class are most profitably preserved in spirit or solution. If they be defended by a soft, flexible, or horny covering, the solution No. I. answers well; if by hard, calcareous plates, the solution No. II. is preferable. They vary in size from microscopic minuteness to upwards of a yard in length. The larger and middle-sized specimens should be kept by themselves, or sewed up in a bag, if placed with others in the same jar or bottle. Rare and beautiful kinds, with transparent glass-like shells, may be captured by the towing-net in tropical seas. The minuter kinds have been commonly neglected, especially those of fresh water; any such species observed darting about in the fresh water of foreign countries should be preserved in tubes, in spirit or solution No. I. The larger kinds of marine crustacea should be suffered to die in fresh water before immersion in the preserving liquor. The different kinds of King-crab (*Limulus*), usually found on sandy or muddy coasts, are particularly worthy of preservation in spirits or solution, with the ova or young.

In preparing Crustacea for drying, care is to be taken to preserve all their external parts as perfect, and as expressive of the natural progressive action, as possible. Crabs and lobsters should be cleaned out as soon as practicable, *i. e.*, the soft internal parts and the flesh should be removed, and they should be soaked in fresh water previous to drying. The claws when large require to be separated at each joint for the purpose, and then refixed, or a small piece may be neatly removed and afterwards replaced. When dried, the specimens should be wrapped in very soft paper, and then packed in cotton, so as not to allow of their being displaced in the case, nor to touch one another. It is desirable, with regard to brilliantly-coloured crabs, to wash them over, after they are dried, with a thin coat of the following varnish :—

Varnish for Crabs, Eggs, &c., No. I.

Common gum, ...	4 oz.
Gum tragacanth, ...	¼ oz.

Dissolve these in three pints of water, add to the solution 20 grains of corrosive sublimate, and 20 drops of oil of thyme, dissolved in four oz. of spirit of wine; mix it well, and let it stand for a few days to separate; the clearer part is to be used as varnish; the thicker part forms an excellent cement.

A very important subject of investigation is the development of the Crustacea, from the earliest period at which they can be observed, to the assumption of the mature or parent form. The eggs, usually of some bright colour, attached beneath the tail of the female crab, lobster, or shrimp, should be examined for this purpose; the embryo, if in course of development, may be readily seen by opening the egg under a moderately magnifying power (see the note on microscopes). Drawings of the different forms or shapes of the embryo should be made, if possible, and the eggs and embryos preserved in spirits or solution in small glass tubes.

Insecta.

Some specimens of all kinds of insects should be preserved, for anatomical examination, in spirit, or the solution No. I. Many of the softer kinds of insects and spiders can only be profitably so preserved. Care must be taken that the softer kinds of insects are not put into the same bottle with the harder kinds. Gauze nets must be used for catching the Lepidoptera (butterflies and moths) on the wing, and a fine muslin net, like a landing-net, for the water insects. Many species may be taken by spreading a cloak, or placing an open umbrella reversed under trees or bushes, and shaking or beating the latter. Caterpillars should be carefully placed in a perforated box with the leaves of the plants on which they are found feeding: they will often undergo their metamorphosis in this captivity; and no *Lepidoptera* are more perfect than those thus *bred*, as it is termed, if carefully watched. The perfect insect should be accompanied, if possible, by its *larva* (caterpillar), and *pupa* (chrysalis or cocoon), together with a specimen of the plant on which it is found feeding. The latter should be kept in an herbarium set apart for the purpose, and should have a number corresponding with that of the insect. Larvæ and pupæ may be preserved in spirit or solution, as well as a specimen of every perfect insect that can be spared, with a view to anatomical investigation. It must be remembered that the larvæ will very soon lose their colours when so treated, and, in order to retain these, a specimen or two of the larger ones and of their pupæ may be opened, the viscera removed, and the inside, after it has been brushed with arseniate soap, stuffed with cotton. Boxes lined with cork are the best conveyances for dried butterflies, moths, and indeed for insects in general; or they may be pinned in the crown of the hat until they can be transferred to a place of safety. The more delicate insects, such as butterflies, moths, sphinxes, the different species of mantis, the locusts, dragon-flies, &c., after being killed by pressure on the thorax, should be pinned down, while in a relaxed state, with the wings and legs kept close to the body, to save space, and prevent collision. The pin should be greased or oiled, to prevent rust; and if pointed at both ends the specimen more readily admits of being turned. The pin should be made fast, so as to allow of the motion of the box in all directions, and the fastening must be adjusted to the weight of the insect. The harder winged insects may be killed by immersion in hot water, and after having been dried on blotting paper, may be laid carefully in boxes upon cotton, so as not to interfere with or injure each other. A ready mode of preserving beetles (*Coleoptera*), when found in abundance on any foreign coast, is to put them, when dried, in a box, in the bottom of which a layer of fine dry sand has been strewed. When the layer is overspread with beetles, they must be covered with another layer of sand, and the packer must proceed with layers of beetles and sand alternately, till the box, which should be water-tight, is quite full, when it should be screwed down, and pitched at the seams. Mr Darwin preserved all his dry specimens of insects, excepting the Lepidoptera, between layers of rag in pill-boxes, placing at the bottom a bit of camphor, and they arrived in an excellent state.

Mollusca (Cuttles, Squids, Snails, land and sea), Slugs (land and sea), Shell-fish, Cowries, Limpets, and Bivalves, as Mussels, Oysters, &c.

" A superficial towing net, another, so constructed as to be kept a fathom or two below the surface, and the deep-sea trawl, are the principal agents for capturing these animals. But when the tide is at the lowest, the collector should wade among the rocks and pools near the shore, and search under overhanging ledges of rock, as far as his arms can reach. An iron rake, with long close-set teeth, will be a useful implement on such occasions. He should turn over all loose stones and growing sea-weeds, taking care to protect his hands with gloves, and his feet with shoes and stockings, against the sharp spines of *Echini*, the back fins of Weavers (sting-fishes), and the stings of *Medusæ* (sea-nettles). In detaching chitons and *patellæ* (limpets), which are all to be sought for on rocky coasts, the surgeon's spatula* will prove a valuable assistant. Those who have paid particular attention to preserving chitons have found it necessary to suffer them to die under pressure between two boards. *Haliotides* (sea-ears), may be removed from the rocks to which they adhere, by throwing a little warm water over them, and then giving them a sharp push with the foot sideways, when mere violence would be of no avail without injuring the shell. Rolled madrepores and loose fragments of rock should be turned over. *Cypraæ* (cowries), and other *Testacea*, are frequently harboured under them. Numbers of *mollusca*, *conchifera*, and *radiata*, are generally to be found about coral reefs." —*Brodrip*.

Among the floating mollusca likely to be met with in the tropical latitudes is the *spirula*, a small cephalopod with a chambered shell. An entire specimen of this rare mollusc is a great desideratum ; and if it should be captured alive, its movements should be watched in a vessel of sea-water, with reference more especially to the power of rising and sinking at will, and the position of the shell during those actions. The chambered parts of the shell should be opened under water, in order to determine if it contains a gas ; the nature of this gas should likewise, if possible, be ascertained. As a part of the shell of the spirula projects externally at the posterior part of the animal, this part should be laid open in the living *spirula*, in order to ascertain how far such mutilation would affect its power of rising or sinking in the water.

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* A case knife, in *experienced hands*, is even a better instrument ; but great care must be taken not to wound the ligamentous border of the shell of the *chitons*, and not to injure the edges of the limpets.

In the event of a living pearly nautilus (*Nautilus Pompilius*) being captured, the same observations and experiments should be made on that species, in which they would be attended with more precision and facility, as the species is much larger than the *spirula*, and its shell external.

The towing-net should be kept overboard at all practicable periods, and drawn up and examined at stated intervals, as some of the rarest marine animals have been taken by thus sweeping the surface of the sea.

A sketch or drawing of molluscous and radiate animals, of which the form and colour are liable to be materially altered by death, or when put in spirit, will aid materially in rendering the description of the species useful and intelligible.

Some of each species should be preserved in spirit, or the solution No. II. If they have died with their soft parts protruded, they should be suspended so as to prevent distortion from pressure. If the shell be of a spiral form, the whorls should be perforated with a fine awl so as to allow the spirit or solution to enter; otherwise, as the main body of the animal fills up the whole mouth of the shell, the deeper seated and softer parts would become putrid before the preserving liquor could get to them.

Where the animal has been detached from its shell, the soft parts and the shell should be marked with corresponding numbers. When the animal is furnished with an *operculum* (the little door which closes the mouth of many turbinated shells), it should be carefully preserved; and if detached from the animal, should be so numbered as to prevent the possibility of its being attributed to the wrong species. Shells should never be cleaned, but should be preserved as they come from the sea, taking care only to fill the mouths of those which are turbinated, with tow or cotton, to prevent fracture. It may be sometimes requisite to put a live shell into hot water and boil it a minute or two, in order to dislodge the animal, which may then be removed with a crooked pin.

The land-shells are found in various situations; as in humid spots covered by herbage, rank grass, &c.; beneath the bark or within the hollows of old trees, crevices of rocks, walls, bones, &c.; about the drainage of houses, or in the dry season by digging near the roots of trees. Early in the morning, especially in rainy weather, is the best time for taking them. The fresh water kinds may be sought for in quiet inlets, on the sides of lakes, rivers, and brooks; the greater number of univalves occur at or near the surface, under the leaves of aquatic plants, and among decayed vegetables; while the bivalves and certain univalves keep at the bottom, and are often more or less embedded in the sand or mud, from which they may be raked into a landing-net.

With regard to the marine bivalves, rocks, sub-marine clay banks, piles, stones, and indurated sand, should be carefully inspected for Pholades, Lithodomi, and other boring species. If the collector should find any of these perforators in the ruins of an ancient temple, or in the remains of any ancient works of art, or any adhering shells (serpules for instance), attached to the surface of such works, the specimens become doubly interesting, especially in a geological point of view. In such cases, the situation should be accurately noted, as well as the distance of the perforations from the surface of the sea, either above or below.

By digging with a wide-pronged fork in sand-banks at low water, many bivalves, such as *Solens*, *Cardia*, *Tellinæ*, &c., will be procured alive; and if the inhabitants of the coast be accustomed to diving, their services should be procured for deeper water. Care must be taken not to separate the ligament which binds the hinge. When the animal is dead the shell will gape, and the soft parts may be then removed without injury. Attempts to open bivalves, while the animals are alive, generally terminate in great injury to the shells.

For deep-sea shells, the dredge is indispensable. Dredging requires experience to judge of the length of rope to be used; if there be too much on a sandy bottom, the dredge will bury itself; if too little, it will not scrape properly; on rocky bottoms the rope must be kept as short as possible; in deep water, the dredge can only be made to act effectually by placing a weight on the line, which as a rule, may be about one-third of the weight of the dredge, and placed on the line at about two-thirds of the depth of the water; the object is to sink the rope, and counteract the tendency it has to float the dredge. The contents of the dredge are best examined by means of sieves, of which three should be used, one over the other, first a riddle, next a wheat sieve, and third an oat sieve; these may be fastened together, the contents of the dredge being emptied into the riddle, and water being poured upon them, the mud, &c., will be washed off and the contents separated, so as to be very easily examined; by this plan, a hundredfold more will be discovered than can be found by searching in mud or sand in the usual manner. Besides shells, numbers of crabs, star-fishes, sea-urchins, worms, corals, zoophytes, algæ, &c., are procured by the dredge.—*The Admiralty Manual of Scientific Enquiry, published by the authority of the Lords Commissioners of the Admiralty*, 1849.

Remarks upon the General Principles of Philological Classification and the Value of Groupes, with particular reference to the Languages of the Indo-European Class. By R. G. Latham, M.D. Communicated by the Ethnological Society.*

In respect to the languages of the Indo-European class, it is considered that the most important questions connected with their systematic arrangement, and viewed with reference to the extent to which they engage the attention of the present writers of philology, are the three following:—

1. *The question of the Fundamental Elements of certain Languages.* The particular example of an investigation of this kind is to be found in the discussion concerning the extent to which it is a language akin to the Sanskrit, or a language akin to the Tamul, which forms the basis of certain dialects of *middle* and even *northern* India. In this is involved the question as to the relative value of grammatical and glossarial coincidences.

2. *The question of the Independent or Subordinate Character of certain Groupes.* Under this head comes the investigation, as to whether the Slavonic and Lithuanic tongues form separate groupes, in the way that the Slavonic and Gothic tongues form separate groupes, or whether they are each members of some higher group. The same inquiry applies to the languages (real or supposed) derived from the Zend, and the languages (real or supposed) derived from the Sanskrit.

3. *The question of Extension and Addition.* It is to this that the forthcoming observations are limited.

Taking as the centre of a group, those languages which have been recognised as Indo-European (or Indo-Germanic), from the first recognition of the group itself, we find the languages derived from the ancient Sanskrit, the languages derived from the ancient Persian, the languages of Greece and Rome, the Slavonic and Lithuanic languages, and the languages of the Gothic stock; Scandinavian, as well as Germanic. The affinity between any two of these groupes has currently been considered to represent the affinity between them all at large.

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* Read before the Society, 28th February 1849.

The way in which the class under which these divisions were contained, as subordinate groupes, has received either *addition* or *extension*, is a point of philological history, which can only be briefly noticed; previous to which a difference of meaning between the words *addition* and *extension* should be explained.

1. To draw an illustration from the common ties of relationship, as between man and man, it is clear that a family may be enlarged in two ways.

a. A brother, or a cousin, may be discovered, of which the existence was previously unknown. Herein the family is enlarged, or increased, by the *real* addition of a new member, in a recognised degree of relationship.

b. A degree of relationship previously unrecognised may be recognised, *i. e.*, a family wherein it was previously considered that a second-cousinship was as much as could be admitted within its pale, may incorporate third, fourth, or fifth cousins. Here the family is enlarged, or increased, by a *verbal* extension of the term.

Now it is believed that the distinction between increase by the way of real addition, and increase by the way of verbal extension, has not been sufficiently attended to. Yet, that it should be more closely attended to, is evident; since, in mistaking a verbal increase for a real one, the whole end and aim of classification is overlooked.

1. *The Celtic.*—The publication of Dr Prichard's Eastern Origin of the Celtic Nations, in 1831, supplied philologists with the most definite addition that has, perhaps, yet been made to ethnographical philology.

Ever since then, the Celtic has been considered to be Indo-European. Indeed its position in the same group with the Iranian, Classical, Slavono-Lithuanic, and Gothic tongues, supplied the reason for substituting the term Indo-*European* for the previous one Indo-*Germanic.*

2. Since the fixation of the Celtic, it has been considered

that the Armenian is Indo-European. Perhaps the well-known affinity between the Armenian and Phrygian languages directed philologists to a comparison between the Armenian and Greek. Müller, in his Dorians, points out the inflexion of the Armenian verb-substantive.

3. Since the fixation of the Celtic, it has been considered that the old Etruscan is Indo-European.

4. Since the fixation of the Celtic, it has been considered that the Albanian is Indo-European.

5. Since the fixation of the Celtic, Indo-European elements have been indicated in the Malay.

6. Since the fixation of the Celtic, Indo-European elements have been indicated in the Laplandic.

7. Since the fixation of the Celtic, it has been considered that the Ossetic is Indo-European.

8. Since the consideration of the Ossetic as Indo-European, the Georgian has been considered as Indo-European likewise.

Now the criticism of the theory which makes the Georgian to be Indo-European, is closely connected with the criticism of the theory which makes the Ossetic and the Malay to be Polynesian; and this the writer reserves for a separate paper. All that he does at present is to express his opinion, that if any of the seven last-named languages are Indo-European, they are Indo-European not by real addition, in the way of recognised relationship, but by verbal extension of the power of the term Indo-European. He also believes that this is the view which is taken, more or less consciously or unconsciously, by the different authors of the different classifications themselves. If he be wrong in this notion, he is at issue with them as to a matter of fact; since, admitting *some* affinity on the part of the languages in question, he denies that it is that affinity which connects the Greek and German, the Latin and Lithuanian.

On the other hand, if he rightly imagine that they are considered as Indo-European on the strength of some other affinity, wider and more distant than that which connects the Greek with the German, or the Latin with the Lithuanic, he regrets that such an extension of a term should have been made without an exposition of the principle that suggested it, or the facts by which it is supported; principles and facts which, when examined by himself, have convinced him that most of the later movements in this department of ethnographical philology, have been movements in the wrong direction.

There are two principles upon which languages may be classified.

1. According to the first, we take two or more languages as we find them, ascertain certain of their characteristics, and then inquire how far these characteristics coincide.

Two or more languages thus taken might agree in having a large per-centage of words in common, or a large percentage of grammatical inflexions; in which case they would agree in certain *positive* characters. On the other hand, two or more such languages might agree in the *negative* fact of having a small and scanty vocabulary, and an inflexional system equally limited; whilst, again, the scantiness of inflexion might arise from one of two causes. It might arise from the fact of inflexions having never been developed at all, or it might arise from inflexions having been lost subsequent to a full development of the same. In all such cases as these, the principle of classification would be founded upon the extent to which languages agreed or differed in certain external characteristics; and it would be the principle upon which the mineralogist classifies minerals. It is not worth while to recommend the adoption of the particular term *mineralogical*, although mineralogy is the science that best illustrates the distinction. It is sufficient to state, that in the principle here indicated, there is no notion of *descent*.

2. It is well known that in ethnographical philology (indeed in ethnology at large) the mineralogical principle is not recognised; and that the principle that *is* recognised is what may be called the *historical* principle. Languages are arranged in the same class, not because they agree in having a copious grammar or scanty grammar, but because they are descended (or are supposed to be descended) from some common stock; whilst similarity of grammatical structure, and glossarial identity are recognised as elements of classification only so far as they are *evidence* of such community

of origin. Just as two brothers will always be two brothers, notwithstanding differences of stature, feature, and disposition, so will two languages which have parted from the common stock within the same decennium, be more closely allied to each other, at any time and at all times, than two languages separated within the same century; and two languages separated within the same century, will always be more cognate than two within the same millennium. This will be the case irrespective of any amount of subsequent similarity or dissimilarity.

Indeed, for the purposes of ethnology, the phenomena of subsequent similarity or dissimilarity are of subordinate importance. Why they are so, is involved in the question as to the rate of change in language. Of two tongues separated at the same time from a common stock, one may change rapidly, the other slowly; and, hence, a dissimilar physiognomy at the end of a given period. If the English of Australia were to change rapidly in one direction, and the English of America in another, great as would be the difference resulting from such changes, their ethnological relation would be the same. They would still have the same affiliation with the same mother-tongue, dating from nearly the same epoch.

In ethnological philology, as in natural history, *descent* is the paramount fact; and without asking how far the value thus given to it is liable to be refined on, we leave it, in each science, as we find it, until some future investigator shall have shewn that either for a pair of animals *not* descended from a common stock, or for a pair of languages *not* originating from the same mother-tongue, a great number of general propositions can be predicated than is the case with the two most dissimilar instances (animal or language) derived from a common origin.

Languages are allied just in proportion as they were separated from the same language at the same epoch.

The same epoch.—The word *epoch* is an equivocal word, and it is used designedly because it is so. Its two meanings require to be indicated, and, then, it will be necessary to ask which of them is to be adopted here.

The *epoch*, as a period in the duration of a language, may be simply *chronological*, or it may be *philological*, properly so called.

The space of ten, twenty, a hundred, or a thousand years, is a strictly chronological epoch. The first fifty years after the Norman conquest is an epoch in the history of the English language; so is the reign of Henry the Third, or the Protectorship of Oliver Cromwell. A definite period of this sort is an epoch in language, just as the term of twenty or thirty years is an epoch in the life of a man.

On the other hand, a period that, chronologically speaking, is indefinite, shall be an epoch. The interval between one change and another, whether long or short, is an epoch. The duration of English like the English of Chaucer, is an epoch in the history of the English language; and so is the duration of English like the English of the Bible translation. For such epochs there are no fixed periods. With a language that changes rapidly they are short; with a language that changes slowly they are long.

Now, in which of these two meanings should the word be used in ethnographical philology? The answer to the question is supplied by the circumstances of the case, rather than by any abstract propriety. We cannot give it the first meaning, even if we wish to do so. To say in what year of the duration of a common mother-tongue the Greek separated from the stock that was common to it and to the Latin is an impossibility; indeed, if it could be answered at once, it would be a question of simple history, not an inference from ethnology: since ethnology, with its palæontological reasoning from effect to cause, speaks only where history, with its direct testimony, is silent.

We cannot, then, in ethnological reasoning get, at the precise year in which any one or two languages separated from a common stock, so as to say that *this separated so long before the other*.

The *order*, however, of separation we *can* get at, since we can *infer* it from condition of the mother-tongue at the time of such separation, this condition being denoted by the condition of the derived language.

Hence the philological epoch is an approximation to the chronological epoch, and as it is nearest approximation that can possibly be attained, it is practically identical with it, so that the enunciation of the principle at which we wish to arrive may change its wording, and now stand as follows,—*Languages are allied, just in proportion as they were separated from the same language in the same stage.*

Now, if there be a certain number of well-marked forms (say *three*) of development, and if the one of these coincide with an early period in the history of language, and another with a later one, and the third with a period later still, we have three epochs wherein we may fix the date of the separation of the different languages from their different parent-stocks; and these epochs are natural, just in proportion as the forms that characterise them are natural.

Again, if each epoch fall into minor and subordinate periods, characterised by the changes and modifications of the then generally characteristic forms, we have the basis for subordinate groups, and a more minute classification.

It is not saying too much to say that all this is no hypothesis, but a reality. There *are* real distinctions of characteristic forms corresponding with real stages of development; and the number of these is three; besides which, one, at least, of the three great stages falls into divisions and subdivisions.

1. The stage anterior to the evolution of inflexion.—Here each word has but one form, and relation is expressed by mere juxtaposition, with or without the superaddition of a change of accent. The tendencies of the stage are to combine words in the way of composition, but not to go further. Each word retains, throughout, its separate substantive independent character, and has a meaning independent of its juxtaposition with the words with which it combines.

2. The stage wherein inflexions are developed.—Here, words originally separate, and afterwards placed in juxtaposition with others, as elements of a compound term, so far change in form, or so far lose their separate signification, as to pass for adjuncts, either prefixed or postfixed to the main word. What was once a word is now the part of a word and what was once Composition is now Derivation, certain sorts of Derivation being called Inflexions, and certain In-

flexions being called Declensions or Conjugations, as the case may be.

3. The stage wherein inflexions become lost, and are replaced by separate words. Here case-endings, like the *i* in *patr-i*, are replaced by prepositions (in some cases by postpositions), like the *to* in *to father ;* and personal endings, like the *o* in *voc-o*, are replaced by pronouns, like the *I* in *I call.*

Of the *first* of these stages, the Chinese is the language which affords the most typical specimen that can be found in the present *late* date of languages—*late*, considering that we are looking for a sample of its earliest forms.

Of the *last* of these stages the English of the year 1849 affords the most typical specimen that can be found in the present *early* date of language—*early*, considering that we are looking for a sample of its latest forms.

Of the second of these stages we must take two languages as the samples.

1. *The Greek.*—Here we have the inflexional character in its most perfect form ; *i. e.*, the existence, as separate words, of those sounds and syllables that form inflexions, is at its maximum of concealment ; *i. e.*, their amalgamation with the primary word (the essence of inflexion) is most perfect.

2. *The Circassian, Coptic, or Turkish.*—In one of these (it is difficult to say which) the existence as separate words of those sounds and syllables which form inflexions, is at its *minimum* of concealment ; *i. e.*, their amalgamation with the primary word (the essence of inflexion) being most imperfect.

This classification is, necessarily, liable to an element of confusion common to all classifications where the evidence is not exactly of the sort required by the nature of the question. The nature of the question here dealt with requires the evidence of the historical kind, *i. e.*, direct testimony. The only evidence, however, we can get at is indirect and inferential. This engenders the following difficulty. The newest language of (say) the languages of the secondary formation may be nearer in chronology, to the oldest language of the third, than to the first formed language of its own class. Indeed, unless we assume the suspension of all change for *long* epochs, and that those correspond with the epochs at which certain languages are given off from their parent stocks, such *must* be the case.

Now, although this is a difficulty, it is no greater difficulty than the geologists must put up with. With them also there are the phenomena of transition, and such phenomena engender unavoidable complication. They do so, however, without overthrowing the principles of their classification.

The position of a language in respect to its stage of development is one thing,—the position in respect to its allied tongues another.

Two languages may be in the same stage (and, *as such*, agree), yet be very distant from each other in respect to affiliation or affinity. Stage for stage the French is more closely connected with the English, than the English with the Mœso-Gothic. In the way of affiliation, the converse is the case.

Languages are allied (or, what is the same thing, bear evidence of their alliance), according to the number of forms that they have in common; since (subject to one exception) these common forms must have been taken from the common mother-tongue.

Two languages separated from the common mother-tongue, subsequent to the evolution of (*say*) a form for the dative case, are more allied than two languages similarly separated anterior to such an evolution.

Subject to one exception. This means, that it is possible that two languages may appear under certain circumstances more allied than they really are, and *vice versâ.*

They may appear more allied than they really are, when, after separating from the common mother-tongue during the ante-inflexionary stage, they develop their inflexions out of the same principle, although *independently.* This case is more possible than proved.

They may appear less allied than they really are, when, although separated from the common mother-tongue after the evolution of a considerable amount of inflexion, each taking with it those inflexions, the one may retain them, whilst the other loses them *in toto.* This case also is more possible than proved.

Each of these cases involves a complex question in philology:—the one the phenomena connected with the *rate of change;* the other the *uniformity of independent processes.*

These questions are likely to affect future researches more than they had affected the researches hitherto established. Another question has affected the researches hitherto established more than it is likely to affect future ones. This is the question as to the *fundamental unity, or non-unity of language.* Upon this the present writer has expressed an opinion elsewhere. At present he suggests that the more the general unity of the human language is admitted, the clearer will be the way for those who work at the details of the different affiliations. As long as it is an open question, whether one class of languages is *wholly* unconnected with others, *any* connection engenders an inclination to arrange it under the group previously recognised. I believe that this determined the position of the Celtic in the Indo-European group. I have great doubts whether if *some* affinity had been recognised from the beginning, it would even have stood where it now does. The question, when Dr Prichard undertook his investigations, was not so much whether the Celtic was in the exact ratio to any or all of the then recognised European languages in which they were to each other, but whether it was in any relation at all. This being proved, it fell into the class at once.

The present writer believes that the Celtic tongues were separated from their mother-tongue at a comparatively early period of the second stage; *i. e.*, when but few inflexions had been evolved; whilst the Classic, Gothic, Lithuano-Slavonic (Sarmatian), and Indo-Persian (Iranian) were separated at comparatively late periods of the same stage, *i. e.*, when many inflexions had been evolved.

Hence he believes that, in order to admit the Celtic, the meaning of the term Indo-European was extended.

Regretting this (at the same time admitting that the Celtic tongue is more Indo-European than any other), he believes that it is too late to go back to the older and more restricted use of the term; and suggests (as the next best change), the propriety of considering the Indo-European class as divided into two divisions, the older containing the Celtic, the newer containing the Iranian, Classical, Sarmatian, and Gothic tongues. All further extensions of the term he believes to be prejudicial to future philology; believing also that all supposed additions to the Indo-European class have (with the exception, perhaps, of the Armenian) involved such farther extension.

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Note.—After the statement of the preceding remarks, Mr Kingsford suggested the possibility of languages becoming wholly uninflexional, and, as such, reduced to a condition like the languages of the first period, in which case they might (as in a *cycle*) undergo a second series of similar developments *de novo;* and so on, *ad infinitum.* This the present writer believed to be a philological possibility ; indeed, in his Inaugural lecture at University College, he had expressed a similar notion.

Note.—Since this was written, a heavy loss has fallen upon the learned world, in the death of Dr Prichard. This induces me to insist more strongly than I should otherwise have done, upon the exception taken to his position of the Celtic being more *verbal* than *real.* High as I put his work upon the Physical History of Mankind (especially as it appeared in the first edition, where, though less learned, it was more critical, more original, and more in advance of contemporary thinkers, than in its final form), I put his Eastern Origin of the Celtic Nations equally high ; and, as a definite addition to ethnographical philology, even higher.

On the Fall of Rivers, especially that of the Jordan, in Palestine; the Thames, Tweed, Clyde, and Dee, in Britain; and the Shannon, in Ireland.

Mr Augustus Petermann, the geographer, in an interesting paper, read to the Geographical Society of London, "On the Depression of the Dead Sea, and the Fall of the Jordan," communicates the following observations on the fall of rivers in general, and on that of the Jordan, in Palestine, of the Thames, Tweed, Clyde, and Dee, in Britain, and the Shannon, in Ireland.

The data regarding the British rivers, Mr Petermann informs us, " are a part of the results of laborious researches into the physical and statistic geography of the British Isles, which he hopes ere long to lay before the public in a series of maps.

1. *The Fall of Rivers.*—This interesting branch of physical geography presents, in its comparative results, such striking anomalies as perhaps have never before been anticipated. There are rivers in this country that are of the same aggregate size and descent, and the one forms in its course a series of considerable waterfalls, the heights of some of which approach nearly to 100 feet; whilst the other, equivalent, as before mentioned, in size and fall, presents not even a single waterfall or cataract, nay, not even one decided rapid.

To ascertain the rate of fall, there are two points, the accurate knowledge of which is necessary,—1st, The length of the river in question; 2d, Its elevation.

The length of the river, that is to say, the development of its course, is greatly influenced by the extent of its windings. These windings, in their true extent, can only be delineated with sufficient accuracy in maps of a very large scale, such as is adapted for national maps, as the windings which rivers generally exhibit disappear in reduced maps to such a degree that a great difference is produced in the calculations.

For the purpose of forming a judgment of the rate of the decrease of a river's course by constructing different maps in decreasing scales, I made the following inquiry regarding a river which is not uncommonly meandering in its course. I here allude to a portion of the Severn, from its source to Shrewsbury, and I found the following numerical results:—

	Scale.	Length.	Difference.	
		Miles.	Miles.	Per Cent.
On the Ordnance or National Map	1 mile to 1 inch	81·8		
... Index Map of the Ordnance Map .	10 mile to 1 inch	68·5	13·3	16·3
... Petermann's River Map of the British Isles	25 mile to 1 inch	62·5	19·3	23·6
... Useful Knowledge Society Map of the British Isles . .	42 mile to 1 inch	58·0	23·8	29·1

The first, the Ordnance Map, forms the basis of the other three. By comparing the Index Map with the Ordnance Map itself, the scale of which is ten times larger, the figures shew that the windings of the river in the Index Map disappear to such a degree as to give a decrease of 13·3 miles for a length of 81·8 miles, or 16·3 per cent.; and so a map on a scale 25 times smaller gives a decrease of 23·6 per cent., and a map on a scale 42 times smaller 29·1 per cent.,—that is, measuring off the length of a river in a map of a scale of 42 miles to 1 inch, the results, however carefully measured, are nearly one-third too small.

Thus with regard to the Jordan, although not a meandering river, and forming almost a straight line from the Lake of Tiberias to the Dead Sea, yet the few bendings and windings of the river, when taken into account, give it a length of 80 English miles,* which is 11 miles longer than Professor Robinson's statement of 60 geographical miles. The 14·3 feet of fall per mile which he thus calculates, will be reduced by the 80 English miles to 12·3 feet. And I have no doubt that when the course of the Jordan is thoroughly explored, it will exhibit a still greater development, and the rate of fall will be still more reduced.†

The fall of a river influences in part the velocity or force of its current, but not to such an extent as that the rate of fall can be taken as a scale for the rate of the velocity and force of the current. We call the Danube, the Rhine, and the Elbe, very rapid rivers, and they only exhibit a fall of 1 and 2 feet, and very seldom 3 feet, per mile; but we should not place the Tweed in the same rank of velocity, and yet it has an average fall of nearly 8 feet in its main course from the point of affluence of Biggar Water to the sea, which is a length of 80 miles, and with this fall is freely navigated by small boats, at least so far up as Peebles;* while a descent of only 2 feet in the Danube presents the greatest obstacles to navigation. Thus the mighty Amazon falls but 12 feet in the last 700 miles of its course, or one-fifth of an inch per mile; yet, from the immense volume of its waters, the collision of its current with the tide of the Atlantic is of the most tremendous description.

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* According to Robinson's Map, in his Biblical Researches.

† The Jordan, as appears from Lieut. Molyneux's account, is extremely winding in its course; the American expedition under Captain Lynch found that it serpentines 200 miles, and the fact confirms Mr Petermann's argument.—*Editor of Journal of Geographical Society.*

It is obvious from the preceding remarks, that, in comparing the rate of fall of one river with another, the size, that is, the depth and width, of the rivers, should be taken into account.

The Jordan in point of magnitude ranks with several rivers of Great Britain. Legh compares the Jordan to the Thames below Oxford; and it appears, in collecting all the different statements for the size of the Jordan between the two lakes, that the average breadth may be considered to be about 30 yards, and the depth 8 or 9 feet.† The Dee of Aberdeenshire ranks in size with the Jordan, although it is only the twenty-ninth of the British rivers, in regard to the extent of its basin, and stands also far below the Jordan in this point of comparison; but this difference is balanced by the nature of the climate in which the two basins are situated. The basin of the Dee lies in a very humid region, as it drains a part of those mountainous districts of Scotland which give birth to so many perennial streams; whilst the basin of the Jordan lies in a region so arid that its tributaries are almost dried up during the summer season; and amongst those which it does receive from the eastern water-slopes of Palestine Proper, there is not one perennial stream.

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* Fullerton's Parliamentary Gazetteer of Scotland, vol. ii., p. 775.

† *Kitto's* Pictorial History of Palestine, vol. ii., p. clxxiv.

I conclude, from various measurements of the breadth and depth of the Dee, that it conveys as great a quantity of water (if not greater) to the German Ocean, as the Jordan to the Dead Sea. The mean annual breadth of the river Dee at Glenmuick parish is 70 yards, and the mean depth 4 feet; the portion of the river within this parish is 38 to 53 miles distant from its mouth; in other parts, further down, the breadth is 80 yards, and the depth 12 feet: thus the average breadth, in its lower course, for a distance of about 50 miles, may be taken at 70 yards, and the depth from 4 to 12 feet;* and as the average size of the Jordan between the two lakes is only computed at 30 yards broad, and 8 to 9 feet deep, the river Dee may fairly be ranked with the Jordan in point of magnitude.

The entire length of the Dee is 87·5 English miles, and its fall 4060 feet.† The greatest portion of this fall naturally belongs to the upper course, the limit of which and the middle course may be best fixed at a distance of 15·3 miles from the source; that is, at the Linn of Dee—a spot where the river has cut through opposing rocks a long narrow passage between 30 and 40 feet deep, and forms four small waterfalls, the central one about 10 or 12 feet, the others not above half that height. Below the fall the water has scooped out a series of basins, in which it rests, deep, dark, and motionless.

From the Linn of Dee to the sea, a distance of 72·2 miles, it has yet to descend 1190 feet, which makes an average fall of 16·5 feet per mile. This is about one-fourth more than the average descent of the Jordan; and yet in the whole course of the Dee below the Linn it does not present a single waterfall or decided rapid. In some places the Dee exhibits even a fall of more than 20 feet per mile, owing to the unequal distribution of fall, to which all rivers are more or less subject, but still there are *no waterfalls* or other sudden descents.

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* New Statistical Account of Scotland, vol. xii., pp. 776, 832, 875.

† Ibid., p. 648. The Dee takes its rise high up on Mount Braeriach, the top of which is 4220 feet high; the well or fountain whence the river springs is 4060 feet, according to Dr Skene Keith.

There is another river in Scotland, the fall of which, although larger, approaches nearly to that of the Jordan. This is the famous border stream, the placid Tweed. The entire length of this river is 96·4 English miles, and the total descent is 1500 feet; thus the average fall per mile would be about 16 feet; but the actual distribution of fall for the different portions of the river is thus:—

	County.	Length.	Height.	Fall per Mile.
		Engl. M.	Engl. ft.	Feet.
From the source . . .	Peebles .	...	1500	...
To affluence of Biggar Water	...	16·5	605	54·2
Altarstone . . .	...	1·6	582	14·2
Neidpath Castle (1 mile before Peebles) .	...	8·0	518	8·0
Cardrona Mains . .	...	6·4	472	7·2
Boundary between the Counties of Peebles and Selkirk . .	...	6·3	400	11·4
Affluence of Cadon Water	Selkirk .	5·0	362	7·6
Fairnielee Bridge . .	..	1·3	348	10·8
Affluence of Gala Water	Roxburgh	4·4	301	10·7
Affluence of Leader Water	Berwick .	4·5	260	9·1
Kelso Bridge . .	Roxburgh	15·4	110	9·7
The Sea	Berwick .	27·0	0	4·1
Total length and fall =		96·4	1500	...

The affluence of Biggar Water is a very good limit between the upper and middle course of the Tweed; the distance from that point to the sea is 80 miles, which is nearly the same as the Jordan between the two lakes. We see from the preceding table that the fall of the Tweed within its middle and lower course approaches in many places very mearly to the supposed average fall of 12 feet in the Jordan, as near as 11·4 feet; and in a short distance from Biggar Water to Altarstone, the rate of fall reaches even 14·2 feet per mile; and yet from the affluence of Biggar Water to the sea the Tweed possesses neither waterfalls nor rapids; and small boats, such as are used in salmon-fishing, are freely navigated.

Thus the fall of the Jordan of 12 feet per mile, even with*out water*falls, does not present such a great contrast to the falls of rivers in general, as is shewn by the adduced instances.

Lieutenant Symond's measurements may therefore prove perfectly correct; and it is not at all necessary that any falls should be discovered to account for the descent of the Jordan.

But certainly there must be something to account for the striking anomaly of Professor Robinson's results and my own. The rivers which he draws up for comparison with the Jordan exhibit more or less falls, and the two rivers which I adduce here exhibit none. One might naturally suppose that this was the consequence of incorrect data on either side, upon which our results are based; but it is not so—it is the anomaly which this feature of hydrographical development exhibits; and it is only from the deficiency of study in this branch of physical geography, or rather the scarcity of data for such a study, that this anomaly has not been fully explained.

As the velocity of rivers does not altogether depend upon the rate of their descent, in like manner the average fall does not determine the formation of cataracts. It is much more the geological character of the country through which the river runs which causes those sudden descents; and countries where sudden declivities abound are chiefly of primary or transition formation. We find striking examples in every direction.

The Severn and the Shannon, for example, are much alike in magnitude. The latter descends, from Lough Allen to its mouth, a distance of 213·8 English miles, 161 feet; the Severn, from Newtown to its mouth, a distance of 210 miles, descends 465 feet. This gives an average descent per mile of 9 inches for the Shannon, and 26·6 inches for the Severn. And yet the Severn pursues its course to the sea without any rapids or falls; whilst the Shannon, with its average fall of one-third less than the Severn, forms those magnificent Rapids of Doonas, which, for grandeur and effect, rank with the most celebrated European waterfalls.

The Tweed and the Clyde exhibit a still more remarkable anomaly. Both are very much alike in point of magnitude: the extent of their basins is 1870 English square miles for the Tweed, and 1580 for the Clyde; and they are still more alike in point of *their* aggregate length and fall: the length of the Tweed being 96·4 miles, and its total fall 1500 feet; the length of the Clyde 98 miles, with 1400 feet of fall. Indeed both rivers for many miles from their source flow nearly in one direction, never diverging to any great distance from each other; and so long as they continue nearly parallel, they flow almost upon the same level, and keep on a high table-land of country, as if hesitating whether to mingle their waters or to remain separate, and whether to turn their courses to the eastern or western slope. Thus they pursue their sympathetic career till near Biggar, when they terminate their upper course, and like two wanderers descending from the mountains together, and separating by cross-roads when they have reached the low country, they at last part, the one turning eastward and the other westward. At this spot the rivers are only 6½ miles distant from each other—are on the same level, and have the same distance to travel ere they reach the sea—yet what a difference in their descent; the Tweed pursues its course evenly and gently; while the Clyde has not parted from its former companion for a greater distance than 18 miles, before it boldly dashes over a whole series of those well-known falls, the principal of which are the Bonnington and Stonebyres Fall, and Corra Linn. The descent over all these falls is computed at 230 feet.*

Thus the preceding examples sufficiently attest that the occurrence of cataracts and other sudden descents in a river depends but little on its aggregate fall.

Thus there is certainly "room in the Jordan for *three cataracts*, each equal in height to the Niagara," as Professor Robinson remarks; but, on the other hand, if there should not be discovered one single rapid in it, there is still nothing of a remarkable phenomenon about it.

The different points on the subject thus adduced are recapitulated as follows:—

1*st*, Lieutenant Symonds' results for the depression of the Dead Sea (1312 feet), compared with the different barometrical results, do not prove such an amount of discrepancy as to justify a doubt in their accuracy.

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* Fullarton's Parliamentary Gazetteer of Scotland, vol. i., p. 232.

2d, The same results for the Lake of Tiberias are so much at variance with the barometrical results, that it seems probable the latter (756 feet) would prove nearer to the true level.

3d, The fall of the Jordan of 984 feet between the two lakes, as computed from the trigonometrical results, does not exhibit an " immense contrast with all similar phenomena." And, moreover, owing to our present defective knowledge of the entire course of the Jordan, and the anomaly of the fall of rivers in general, inferences drawn from the aggregate fall of the Jordan can scarcely prove of sufficient weight to control the results of trigonometrical operations.

I now beg leave to add a few data on the fall of some rivers in this country, selecting such as might give a view of the great variety of descents.

The British Isles afford a wide field for the study of hydrography; and I have no hesitation in saying that it possesses for that study such extensive and valuable materials and data as no other country can boast of; but, comparatively speaking, these materials have been drawn forward, sifted, and made applicable for the promotion of science to a very limited extent. For example, the country has been levelled in all directions, and especially along all rivers of note, for the purpose of laying out canals and railways, and yet we find little information in any work regarding general results of the comparative fall of rivers.

The levels in the following data are based upon authentic documents, and relate entirely to the surface of the river above the sea at low water. The distances have been for the first time attempted to be ascertained with accuracy.

I. *The Shannon.*—This is the third largest river in the United Kingdom, in regard to its basin.* As the fall of rivers in general is greatest at their sources, and decreases proportionately towards their mouths, the Shannon in its descent presents one of the rare exceptions to the general fall of rivers, as it is greater in its lower than in its upper course. Its source, the Shannon Pot, or more generally called Legnashinna, rises in the county of Cavan, between Upper Lough

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* The two larger rivers are the Humber (including the Trent and the Ouse) and the Severn.

Macnean and Lough Allen, and is 345 feet above the sea. After a course of 11·6 miles, it enters Lough Allen, up to which it is rendered navigable, owing to the little descent from that place. The distance to which it is navigable is 213·8 miles from its mouth; and in this respect it is superior to all other British rivers—the navigation of the Severn extending only 192, and that of the Thames 193 miles from their mouths. From the head of Lough Allen to the foot of Lough Derg, a distance of 131·8 miles, it descends only 50·5 feet, or 4½ inches per mile. After leaving Lough Derg, the inclination of its course changes considerably, and gradually increases, till it reaches a fall of nearly 17 feet per mile between the town of Castle Connel and Castle Troy, a distance of 3·3 miles. It is in this portion where the mighty Shannon, 40 feet deep and 300 yards wide, forms the magnificent Rapids of Doonas.

The Fall of the Shannon.

	County.	Length.	Height.	Fall per Mile.
		Engl. M.	Engl. Ft.	Feet.
From source, Legnashinna .	Cavan .	...	345	...
To Lough Allen, entrance .	Leitrim .	11·6	161	15·9
Lough Allen, issue . .	...	7·0	161	...
Carrick-on-Shannon, Bridge	...	11·5	155	0·5
Lough Boderg and Lough Bofin, entrance . .	...	12·0	131	2·0
Lough Boderg and Lough Bofin, issue . . .	...	4·7	131	...
Lough Forbes, entrance .	Longford .	4·6	128	0·7
Lough Forbes, issue .	...	2·2	128	...
Lough Ree, entrance (at the Bridge of Lanesborough . . .	...	10·0	125·5	0·2
Lough Ree, issue (1·8 miles above the Bridge of Athlone) . . .	Roscommon	17·5	125·5	...
Shannon Bridge . .	...	16·7	116	0·6
Lough Derg, entrance (1 mile S. from Portumna Bridge) . . .	Tipperary	22·6	110·5	0·2
Lough Derg, issue (1·1 mile N. from Bridge of Killaloe) . . .	...	23·0	110·5	...
Castle Connel . . .	Limerick .	9·8	89·8	2·1
Castle Troy . . .	...	2·3	34·5	16·8
Limerick, Southern Bridge	...	6·4	13·5	3·3
The Sea, between Loop and Kerry Head . .	Clare . .	62·5	0	0·2
Total length =		224·4	...	...

The Fall of the Thames.

	County.	Length.	Height.	Fall per Mile.
		Engl. M.	Engl. Ft.	Feet.
From the source, Thames Head (1¼ miles N. of Kemble)	Wilts . .	...	376·3	...
To affluence of Coln (0·8 miles SW. of Lechlade Bridge)	Gloucester	22·0	243·1	6·1
Tadpole Bridge, near Bampton . .	Oxford .	10·5	219·3	2·3
Skinner's Wear (¾ mile S. from Ensham Bridge)	...	11·8	203·5	1·3
Oxford Bridge to Botley	...	7·8	190·0	1·7
Abingdon Bridge .	Berks . .	9·2	168·8	2·3
Clifton Ferry . .	...	6·5	158·0	1·7
Affluence of Kennet at Reading . .	...	25·0	119·7	1·5
Henley Bridge . .	...	8·1	106·0	1·7
Great Marlow Bridge	...	8·0	94·2	1·5
Windsor Bridge .	...	13·7	65·6	2·1
Affluence of Coln at Egham	Surrey .	7·5	52·1	1·8
Affluence of Wey, near Wey Bridge . .	...	7·1	40·8	1·6
Teddington, first lock	Middlesex	13·2	21·0	1·5
London, London Bridge	..	17·6	4·3	0·9
The Sea at Nore Light	Kent . .	47·2	0·	0·1
Total length and fall =		215·2	376·3	...

II. *The Thames.*—This noble river, although the most important in Great Britain in a commercial point of view, is only the fourth in point of magnitude. The entire length of its course is 215·2 miles, which is 9 miles less than the Shannon; its descent is 376·3 feet. Unlike the Shannon, it has a more equally distributed fall throughout its course; from its head to Lechlade, a distance of 22·0 miles, its fall is 6 feet; and from thence to its mouth its average fall is only 1·2 feet per mile.*

III. *The Tweed.*—We have already fully treated of this river in the foregoing observations; in point of area of its basin it ranks ninth amongst British rivers.

IV. *The Clyde.*—My results respecting the fall of this and the preceding river are almost entirely based upon levellings;

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* From Teddington to London Bridge it is 16 ft. 9 in. at low, and 1 ft. 6 in. at high water.

the sources only of the two rivers are ascertained barometrically, and the portion of the Clyde comprising the falls has likewise not been ascertained by levelling instruments; however, that does not influence the accuracy of the general results, as the exact levels of the Clyde *above* and *below* the Falls check the intervening portion.

The Fall of the Clyde.

	County.	Length.	Height.	Fall per Mile.
		Engl. M.	Engl. Ft.	Feet.
From source . . .	Lanark .	...	1400	...
To Clydesburn, near Little Clyde House . . .	...	1·8	999	222·8
Bodsberryside . . .	...	3·0	872	42·3
Crawford . . .	...	3·7	807	17·6
Affluence of Duneaton Water	...	5·0	734	14·6
Hardington Hall .	...	3·7	686	13·0
Wolf Clyde Bridge near Biggar . . .	...	6·0	632	9·0
Eastfield	...	8·4	605	3·2
Bonnington Fall, *above* the Fall (height of fall 30 feet)	...	10·0	? 400	20·5
Corra Linn (height 84 feet) *above* the Fall . .	...	0·7	365	7·1 †
Stonebyres Fall (height 80 feet) *below* the Fall* .	...	3·0	170	10·3 †
Redleewood, near Daldowie House	...	20·2	46	7·6 †
Glasgow, Glasgow Bridge	...	(9·0)	?	?
Sea at Dumbarton . .	Dumbarton	23·5	0	1·4
Total length and fall =		98·0	1400	...

The accuracy of the data both for the Clyde and Tweed, which I ascertained from two lines of levellings quite independent of each other, are checked by a phenomenon which it might not be uninteresting to record in this place.

Both rivers are very nearly at the same level near Biggar. This very spot exhibits the remarkable phenomenon of a bifurcation of the two rivers—a bifurcation which differs from other larger (and more important) examples only so far as to depend upon the state of water in the Clyde. I give the words of the attentive angler who describes it :*—"It is a singular circumstance that salmon and their fry have occasionally been taken in the upper parts of the Clyde, above its loftiest fall, which, being 80 feet in height, it is utterly impossible for fish of any kind to surmount. The fact is accounted for in this way. After passing Tinto Hill, the bed of the Clyde approaches to a level with that of the Biggar Water, which is close at hand, and discharges itself into the Tweed. On the occasion of a large flood the two streams become connected, and the Clyde actually pours a portion of its waters into one of the tributaries of the Tweed, which is accessible to and frequented by salmon."

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* There are two other falls of smaller dimensions, viz., the Dundaf Linn, ¼ mile below Corra Linn, 4 feet high; and one which is ½ mile below Bonnington Fall. The total descent of the river from the first to the last fall—a distance of 3·7 miles,—amounts to 230 feet.

† *The Falls* excluded.

V. *The Dee.*—We have already noticed at some length the fall of this river. The results for the Dee I have based upon levels ascertained by repeated barometrical measurements by Dr Skene Keith and Dr Dickie of Aberdeen, which have been kindly communicated to me by the latter. This gentleman also confirms my statements by his personal knowledge of the Dee—that it does not exhibit any cataract from its mouth up to the Linn.

The Fall of the Dee.

	County.	Length.	Height.	Fall per Mile.
		Engl. M.	Engl. Ft.	Feet.
From source	Aberdeen .	...	4060	...
To Affluence of Garchary	...	4·3	1984	482·8
Affluence of Guisachan	...	3·0	1640	114·7
Affluence of Geauly	...	5·0	1294	69·2
Linn of Dee	...	3·0	1190	34·7
Ballater Bridge	...	26·4	780	15·6
Belwade, Bridge (not in maps)	...	...	(310)	...
Banchory-Ternan (affluence of Feugh W.)	Kincardine	26·0	172	23·4
Drumoak	Aberdeen	7·2	90	11·4
Affluence of Coulter Burn	...	3·8	60	7·9
Sea at Aberdeen	...	8·8	0	6·8
Total length and fall =		87·5	4,060	...

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* Stoddart's Angler's Companion for Scotland.

The source of the Dee, rising between Ben MacDhui on the east and Braeriach on the west, is 4060 feet high, and most probably it is the highest source in the United Kingdom. The highest spring on Ben Nevis is only 3602 feet, according to my barometrical measurements—that is, 766 feet below the top of the hill; another spring, on one of the highest hills of the Grampians, Ben Aulder, reaches a height of 3650 feet.

An Analysis of Plate-Glass. By Messrs J. E. Mayer and J. S. Brazier.

In going over the analyses of the different varieties of glass which have been recorded, we find that but little attention has been paid to the composition of plate-glass, a material which is almost becoming a necessary of life. It is, moreover, remarkable that no analysis of the plate-glass manufactured in Great Britain has ever been published.

The following pages contain the results obtained from the analyses of three different specimens of plate-glass, which we undertook at the request of Dr Hofmann.* These specimens were procured at the three most extensive plate-glass manufactories of England, which are,

1. The British Plate-Glass Company, St Helens, Liverpool.
2. The London Thames Plate-Glass Company, Bow Creek, Blackwall.
3. The London and Manchester Plate-Glass Company, Sutton, St Helens, Liverpool.

For the purpose of analysis, these specimens of glass were reduced to the most minute state of division, which was effected by levigating in the usual manner. None of the specimens, whilst digesting in water, gave any reaction with the most delicate test-papers.

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* I am indebted for these specimens to the kindness of Mr Fincham of the *British* Plate-Glass Works.—*Dr* A. W. *Hofmann*.

To determine the extent of their solubility in water, from four to five grammes were digested in that menstruum for about forty-eight hours, the clear solution in each case yielded on evaporation but a slight residue, too small for determination.

The specific gravity of these specimens of glass is as follows:—

British Plate-Glass, . . .	2·319
London Thames Plate-Glass, . .	2·242
London and Manchester Plate-Glass, .	2·408

A qualitative examination shewed the presence of silicic acid, potash, soda, sesquioxide of iron, alumina, lime, and, in one case, traces of manganese.

The silicic acid was determined in the usual manner by fusion with pure carbonate of potash. The sesquioxide of iron, the alumina, and the lime, were afterwards precipitated from the hydrochloric filtrate.

To determine the alkalies the glasses were decomposed by means of hydrofluoric acid, in an apparatus recommended by Brunner,* which consists of a leaden capsula with a flat bottom, about 6 inches in diameter, and 4 inches high, in the centre of which is placed a small leaden ring about an inch and a half high, which serves as a support for a platinum dish. The leaden capsula has a cover fitting perfectly tight.

To set the apparatus in action it is necessary to cover the bottom of the capsula with a layer of pulverised fluor-spar about half an inch in thickness, and to pour upon it some sulphuric acid, sufficient to form a thick paste. A weighed portion of the finely-powdered glass, after being put in the platinum dish, is covered with water, and placed on the leaden ring. The whole is then kept at a gentle heat either on a sand-bath, or by means of a spirit-lamp.

By a few preliminary experiments we found the action on the glass to be exceedingly slow when covered merely with water; it was then suggested to us by Dr Hofmann to try, instead of water, a strong solution of ammonia; we found that the hydrofluoric acid being much more rapidly absorbed by this latter agent, the decomposition was facilitated in a remarkable manner.

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* Poggendorff's Annalen, xliv., p. 134.

The first of the two following Tables shews the amount of substance employed; the results obtained are exhibited in Table II.

Table I.

	I. British Plate-Glass.		II. London Thames Plate-Glass.		III. London and Manchester Plate-Glass.	
	1. Grm.	2. Grm.	1 Grm.	2 Grm.	1 Grm.	2 Grm.
Quantity of Glass for general analysis, .	1·3429	1·1750	1·1579	1·1906	1·0508	1·1095
Quantity of Glass for estimation of alkalies, . . .	1·9400	2·1500	1·4200	1·6800	1·0200	2·0700

Table II.

	I. British Plate-Glass.		II. London Thames Plate-Glass.		III. London and Manchester Plate-Glass.	
	1 Grm.	2 Grm.	1 Grm.	2 Grm.	1 Grm.	2 Grm.
Silicic acid, . .	1·0402	0·9180	0·9090	0·9300	0·8200	0·8630
Chlorides of Potassium and Sodium, .	0·5700	0·6460	...	...	0·2675	0·5360
Bichloride of Platinum and Potassium,	0·3100	0·3610	...	...	0·0925	0·1835
Chloride of Sodium, .	0·4735	0·5360	...	...	0·2390	0·4790
Sesquioxide of Iron and Alumina, .	0·0127	0·0105	0·0320	0·0495	0·0373	0·0405
Carbonate of Lime, .	0·1266	0·1135	0·1245	0·1305	0·0887	0·0987
Sulphates of Potash and Soda, . .	...	...	0·4105	0·4940	...	...
Sulphate of Baryta, .	...	...	0·6645*	0·7960	...	...

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* These numbers were obtained in an indirect determination of the alkalies.

The following numbers correspond with the foregoing results:—

I.—*British Plate-Glass.*

	I.	II.	Mean.
Silicic Acid,	77·4592	77·2700	77·3646
Potash,	2·8110	3·2192	3·0151
Soda,	12·9232	13·2028	13·0630
Lime,	5·2192	5·4096	5·3144
Manganese,	...	...	...
Sesquioxide of iron,	0·9457	0·8936	0·9197
Alumina,	trace.	trace.	trace.
	99·3583	99·9952	99·6768

II.—*London Thames Plate-Glass.*

	I.	II.	Mean.
Silicic acid,	78·5050	78·8669	78·6859
Potash,	1·2744	1·4176	1·3460
Soda,	11·5919	11·6724	11·6322
Lime,	6·0605	6·1380	6·0992
Manganese,	...	...	...
Sesquioxide of iron,	trace.	trace.	trace.
Alumina,	2·7636	2·5970	2·6803
	100·1954	100·6919	100·4436

III.—*London and Manchester Plate-Glass.*

	I.	II.	Mean.
Silicic acid,	78·0357	77·7827	77·9092
Potash,	1·7453	1·7062	1·7257
Soda,	12·4373	12·2822	12·3598
Lime,	4·7270	4·9816	4·8543
Manganese,	traces.	traces.	traces.
Sesquioxide of iron,	...	...	...
Alumina,	3·5495	3·6502	3·5998
	100·4948	100·4029	100·4488

A Table is subjoined, containing analyses of several varieties of plate-glass, in order that the composition of the plate-glass in this country may be compared with that manufactured abroad. The Venetian glass was analysed by M. Berthier, the Bohemian mirror-glass by Peligot, and the French glasses by Dumas.*

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* Comp. Knapp's Technology, vol. ii., p. 16.

	Venetian Plate-Glass.	Bohemian Plate-Glass.	French Plate-Glass.		British Plate-Glass	London Thames Plate-Glass.	London and Manchester Plate-Glass.
			No. 1.	No. 2.			
Silicic Acid,	68·6	67·7	75·9	73·85	77·36	78·68	77·90
Potash, .	6·9	21·0	...	5·50	3·01	1·34	1·72
Soda, . .	8·1	...	17·5	12·05	13·06	11·63	12·35
Lime, .	11·0	9·9	3·8	5·60	5·31	6·09	4·85
Magnesia, .	2·1	...	...	...	...	...	...
Manganese,	0·1	...	...	...	...	...	trace
Oxide of Iron,	0·2	...	...	...	0·91	trace	...
Alumina, .	1·2	1·4	2·8	3·50	traces	2·68	3·59
	98·2	100·0	100·0	100·00	99·65	100·42	100·41

Plate-glass is usually considered as a double silicate of lime and soda, or of lime and potash. The following atomic expressions represent the different analyses contained in the above table; the amount of potash contained in the English varieties of glass being very trifling, this oxide has been neglected altogether in the construction of their formulæ.

Venetian plate-glass, .	$2\,KO$,	$3\,NaO$,	$5\,CaO$,	$22\,SiO_3$
Bohemian mirror-glass,	KO		CaO,	$4\,SiO_3$
French plate-glass, No. 1,		$4\,NaO$,	CaO,	$11\,SiO_3$
French plate-glass, No. 2,	KO,	$3\,NaO$,	$2\,CaO$,	$14\,SiO_3$
British plate-glass, .		$2\,NaO$,	CaO,	$9\,SiO_3$
London Thames plate-glass,		$2\,NaO$,	CaO,	$8\,SiO_3$
London and Manchester plate-glass, .		$2\,NaO$,	CaO,	$9\,SiO_3$

—*Quarterly Journal of the Chemical Society*, No. vii. for October 1849, p. 208.

On Carbonate of Lime as an ingredient of Sea-Water. By JOHN DAVY, M.D., F.R.S. Lond. and Edin., Inspector-General of Army Hospitals, &c.

The manner in which limestone-cliffs, rising above deep water, are worn by the action of the sea, as it were by a weak acid, such as we know it contains, viz., the carbonic; the manner, further, in which the sand, on low shores where the waves break, becomes consolidated, converted into sandstone, by the deposition of carbonate of lime from sea-water, owing to the escape of carbonic acid gas, are facts clearly proving that carbonate of lime is, as a constituent of sea-water, neither rare of occurrence, nor unimportant in the economy of nature, inasmuch as the phenomena alluded to,—the one destructive, the other restorative,—have been observed in most parts of our globe where geological inquiry has been instituted.

Reflecting on the subject, it seemed to me desirable to ascertain whether carbonate of lime, as an ingredient of sea-water, is chiefly confined to the proximity of coasts, or, not so limited, enters into the composition of the ocean in its widest expanse.

On a voyage from Barbadoes, in the West Indies, to England, in November last (1848), I availed myself of the opportunity to make some trials to endeavour to determine this, the results of which I shall now briefly relate.

First, I may mention that water from Carlisle Bay in Barbadoes, tested for carbonate of lime, gave strong indications of its presence; thus, a well-marked precipitate was produced by ammonia, after the addition of muriate of ammonia in excess, that is, more than was sufficient to prevent the separation of the magnesia, which enters so largely into the composition of sea-water; and a like effect was produced either by boiling the water, so as to expel the carbonic acid, or by evaporation to dryness, and resolution of the soluble salts.

On the voyage across the Atlantic, the test, by means of ammonia and muriate of ammonia, was employed, acting on about a pint of water taken from the surface. The first trial was made on the 15th of November, when in latitude 20° 30′ north, and longitude 63° 20′ west, more than a hundred miles from any land; the result was negative. Further trials were made on the 22d of the same month, in lat. 32° 53′, long. 45° 10′; on the 24th, in lat. 36° 23′, long. 37° 21′; on the 25th, in lat. 37° 21′, long. 33° 34′; on the 26th, in lat. 38° 28′, long. 30° 2′; on the 27th, when off Funchal of the Western Islands, in lat. 38° 32′, long. 28° 40′, about a mile and a half from the shore, the water deep blue, as it always is out of soundings; in all these instances, likewise, the results were negative; the transparency of the water was nowise impaired by the test applied. The last trial was made on the 3d of December, when in the channel off Portland Head about fifteen miles; now, slight traces of carbonate of lime were obtained, a just perceptible turbidness being produced.

The sea-water from Carlisle Bay, the shore of which and the adjoining coast are calcareous, yielded about 1 per 10,000 of carbonate of lime, after evaporation of the water to dryness, and the resolution of the saline matter. A specimen of water taken up on the voyage off the volcanic island of Fayal, about a mile from land, yielded a residue which consisted chiefly of sulphate of lime, with a very little carbonate of lime,—a mere trace; acted on by an acid, it gave off only a very few minute air-bubbles. A specimen taken up off Portland Head about fifteen miles, yielded an evaporation and resolution of the saline matter only a very minute residue, about 4 only per 10,000; it consisted in part of carbonate, and in part of sulphate of lime.

What may be inferred from these results? Do they not tend to prove that carbonate of lime, except in very minute proportion, does not belong to water of the ocean at any great distance from land? And, further, do they not favour the inference, that, when in notable proportion, it is in consequence of proximity to land, and of land the shores of which are formed chiefly of calcareous rock? In using the word proximity, I would not limit the distance implied to a few miles, but rather to fifty or a hundred, as I am acquainted with shores consisting of volcanic islands in the Caribbean Sea, destitute of calcareous rock, on which, in certain situations, sandstone is now forming by the deposition from the sea-water of carbonate of lime.

Should these inferences be confirmed by more extensive inquiry, they will harmonise well with the facts first referred to, the solvent power, on one hand, of sea-water impregnated with carbonic acid, on cliffs of calcareous rock, in situations not favourable to the disengagement of carbonic acid gas; and the deposition, on the other hand, of carbonate of lime, to perform the part of a cement on sand, converting it into sandstone, in warm shallows, where the waves break under circumstances, such as these are, favourable to the disengagement of this gas; and, I hardly need add, that the same inferences will accord well with what may be supposed to be the requirements of organization, in the instances of all those living things inhabiting the sea, into the hard parts of which carbonate of lime enters as an element.

Apart from the economy of nature, the subject under consideration is not without interest in another relation,—I allude to steam navigation. The boilers of sea-going steam-vessels are liable to suffer from an incrustation of solid matters firmly adhering, and with difficulty detached, liable to be formed on their inside, owing to a deposition which takes place from the salt water used for the production of steam. On one occasion that I examined a portion of such an incrustation taken from the boiler of the "Conway," a vessel belonging to the West Indian Steam-Packet Company, I found it to consist principally of sulphate of lime, and to contain a small proportion only of carbonate of lime. This vessel had been employed previously in transatlantic voyages, and also in intercolonial ones, plying between Bermudas and the island of St Thomas, and in the Caribbean Sea and the Gulf of Mexico.

The composition of this incrustation, like the preceding results, would seem to denote, if any satisfactory inference may be drawn from it, that carbonate of lime is in small proportion in deep water distant from the land, and that sulphate of lime is commonly more abundant. The results of a few trials I have made, whilst rather confirmatory of this conclusion, shewed marked differences as to the proportion of sulphate of lime in sea-water in different situations. That from Carlisle Bay was found to contain 11·3 per 10,000. A specimen taken up in lat. 29° 19′, and long. 50′·45, yielded about 2 per 10,000, with a trace of carbonate of lime. A specimen taken up off Fayal yielded about 9 per 10,000, also with a trace of carbonate of lime. One taken up off Portland Head, about fifteen miles distant, yielded, as already remarked, only 4 per 10,000, part of which was sulphate, part carbonate of lime.

By certain management, I am informed, as by not allowing the sea-water in the boilers to be concentrated beyond a certain degree, the incrustation, in the instances of the transatlantic steamers, is in a great measure prevented. Perhaps it might be prevented altogether, were sea-water never used but with this precaution, and taken up at a good distance from land, and in situations where it is known that the proportion of sulphate of lime is small. If this suggestion be of any worth, further, more extensive and exact inquiry will be requisite to determine the proportion of sulphate of lime in different parts of the ocean, and more especially towards land. By the aid of the Transatlantic Steam Navigation Companies, means for such an inquiry may easily be obtained; and it can hardly be doubted that the results will amply repay any cost or trouble incurred.—(*Proceedings of the Royal Society of London, March* 29, 1849.)

On the Snow-Line in the Himalaya. By Lieutenant R. STRACHEY, Engineers. Communicated by order of the Honourable the Lieutenant-Governor, North-Western Provinces of India.

The height at which perpetual snow is found at different parts of the earth's surface, has become an object of inquiry, not only as a mere physical fact, but as a phenomenon intimately connected with the distribution of heat on the globe. In M. Humboldt's efforts to throw the light of his knowledge on this question, he has, when treating of the Himalaya, been unfortunately led much astray by the very authorities on whom he placed most reliance; and his conclusions, though in part correct, cannot lay claim to any pretension to exactness. That he was, indeed, himself conscious of the deficiencies in the evidence before him, is manifest from his ending his disquisition by a declaration, that it was necessary,

"de rectifier de nouveau et par des mesures bien précises dont toute le détail hypsométrique soit publié, ce qui reste de douteux sur la hauteur comparative des deux pentes de l'Himalaya, sur l'influence de révérbération du plateau Tubétain, et sur celle que l'on suppose au courant ascendant de l'air chaud des plaines de l'Inde. C'est un travail à recommencer." (*Asie Centrale*, t. iii., p. 325.) Men of science will still long have to regret that this illustrious traveller was prevented from visiting the East; Englishmen alone need remember that he was prevented by them.

The result of M. Humboldt's investigations on the position of the snow-line in this part of the Himalaya is thus given by himself:—"The limit of perpetual snow on the southern declivity of the Himalaya chain is 2030 toises (13,000* feet, English) above the level of the sea; on the northern declivity, or rather on the peaks which rise above the Tartarian plateau, this limit is 2600 toises (16,600 feet) from 30½° to 32° of latitude; while, under the equator, in the Andes of Quito, it is 2470 toises (15,800 feet). I have deduced this result from the collection and combination of many data furnished by Webb, Gerard, Herbert, and Moorcroft. The greater elevation to which the snow-line recedes on the Thibetian declivity, is the result conjointly of the radiation of heat from the neighbouring elevated plains, the serenity of the sky, and the infrequent formation of snow in very cold and dry air." —(*Cosmos*, *Trans.*, t. i., p. 363, *note* 5.)

The portion of the Himalaya to which allusion has most generally been made, in treating of the snow-line, is that which lies between the north-western frontier of Nipál and the river Sutlej, and it is solely to this part of the chain that my remarks are intended to apply. It extends from about the 77th to the 81st degree of east longitude, and its entire breadth, from the plains of India on the south to the plains of Thibet on the north, is about 120 miles. The mountains on which perpetual snow is found, are confined within a belt of about 35 miles in width, running along the northern boundary of the chain, and they all lie between the 30th and 32d degree of north latitude.

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* The reduction of toises into English feet is everywhere given to the nearest hundred only.

If we now examine the structure of the mountains more closely (*vide* sheets 47, 48, 65, and 66, of the Indian Atlas), we shall find that from the sources of the Touse (long. 78° 30′) to those of the Káli (long. 81° 0′), a space which includes the provinces of Garhwál and Kumaon, all the great rivers, viz., the Bhágirati, Vishnú-ganga, Dauli (of Niti), Gori, Dauli (of Darma), and Káli, run in directions not far from perpendicular to the general direction of the Himalaya. Further, that they are separated, one from another, by great transverse ranges, on which all the highest of the measured peaks of this region are to be found. It will also be seen that the sources of these rivers* are in the main water-shed of the chain, beyond which a declivity of a few miles leads directly to the plains of Thibet. A line drawn through the great peaks will be almost parallel to the water-shed, but about 30 miles to the south of it.

To the west of the Touse the arrangement of the drainage is very different. From the source of this river an unbroken ridge extends to the Sutlej, almost on the prolongation of the line of the great eastern peaks, but more nearly east and west. On this range, which separates Kunáwar from the more southern parts of Bissehir, and which, as it has hitherto received no distinctive name, I shall call the Bissehir range, are the Rúpin, Gunás, Burendo, and Shátál passes; and no perpetual snow is to be found further south among these western mountains. To the north of this range, and almost parallel to it, run several others of somewhat greater altitude, between which the streams of eastern Kunáwar flow into the Sutlej from south-east to north-west, nearly parallel to the upper, and perpendicular to the lower part of the course of that river.

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* I mean the most distant sources of the tributaries, for several of the rivers that I have mentioned, *nominally* end in glaciers to the south of the water-shed.

If we now follow two travellers into Thibet, one from Kumaon or Garhwál, and the other from Simla, or the western hills, we shall be prepared to find that the circumstances under which they will cross the snowy mountains will be very different. The former will proceed up the course of one of the great rivers before alluded to, and ascending the gorge, by which it breaks through the line of the great peaks, will pass unobserved the true southern limit of the perpetual snow; he will leave the great peaks themselves far behind him, and will finally reach the water-shed of the chain, where he may, possibly for the first time, find glaciers and snow. He will here cross straight into Thibet, from what will appear to him the *southern*, to what he will call the *northern declivity* of the Himalaya.*

The western traveller, on the other hand, will find, almost at his first step, a snowy barrier drawn across his path, and he will naturally suppose that he crosses from the southern to the northern face of the snowy range, when he descends from the Shátúl, or some neighbouring pass, into the valley of Kunáwar; and in this idea he will probably be confirmed, by the total change of the climate which he will perceive, and by his being able to penetrate to Shipke, the frontier village of Thibet in this quarter, without meeting any further obstacle on his road at all comparable to that he has passed, or perhaps even without again crossing snow.†

Without waiting to inquire whether either of our travellers has in fact come to a just conclusion, it will be sufficient for my purpose to point out that they mean totally different things by their north and south declivities; and it will be indeed surprising if they agree as to the position of the snow-line. It is manifest, therefore, that, before we can expect to arrive at any correct results, we must get rid of the confusion caused by the ambiguity of the terms *north* and *south declivity;* terms which, at the best, are very ill adapted to convey definite ideas of position in so vast and complicated a mass of mountains. In spite of every care, they will constantly be liable to misconception, as must always be the case where a restricted signification is arbitrarily applied, in a discussion of this sort, to expressions which of themselves have an extended general meaning.*

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* This does not exactly apply to the passes usually crossed between Júhar and Thibet, which will be mentioned more particularly hereafter. There is a pass, however, the "Lashar," though from its badness it is not used, which affords a direct communication.

† The ordinary route lies up the bank of the Sutlej.

As a substitute for the *declivities*, then, the best standard that occurs to me, to which to refer when alluding to the elevation of the snow-line at any place, is the general mass of perpetual snow, found on the more elevated parts of the Himalaya, *the belt of perpetual snow*, which, as I before stated, is about 35 miles in breadth, and runs along the northern boundary of the chain. Instead of the height of the snow-line on *the northern or southern declivity*, I shall therefore say, the height *at the northern or southern limit of the belt of perpetual snow*, where *the limits of the belt of perpetual snow* are to be understood as having exactly the same relation to the snowy surface in a *horizontal plane* that *the snow-line* has in a *vertical*.

It remains for me to define clearly what is meant by the *snow-line*, and I cannot do better than adopt the words of M. Humboldt, who says, " the lower limit of perpetual snow in a given latitude is the boundary line of the snow which resists the effect of summer; it is the highest elevation to which the snow-line recedes in the course of the whole year. We must distinguish between the limit thus defined, and three other phenomena; viz., the annual fluctuation of the snow-line; the phenomena of sporadic falls of snow, and the existence of glaciers."—(*Cosmos*, *Trans.*, t. i., p. 327.)

Having disposed of these preliminaries, which are essential to the proper apprehension of the subject, I shall proceed to examine the data from which the elevation of the snow-line is to be determined. In doing this, it will, I think, be more convenient for me, both for the northern and southern limits, to explain, first, my own views, and afterwards to follow M. Humboldt's authorities, and point out the errors into which they have fallen.

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* *As a specimen*, *vide* Captain Hutton's Papers, noticed hereafter.

1. *Southern limit of the belt of perpetual snow.*—In this part of the Himalaya, it is not, on an average of years, till the beginning of December, that the snow-line appears decidedly to descend for the winter. After the end of September, indeed, when the rains are quite over, light falls of snow are not of very uncommon occurrence on the higher mountains, even down to 12,000 feet; but their effects usually disappear very quickly, often in a few hours. The latter part of October, the whole of November, and the beginning of December, are here generally characterised by the beautiful serenity of the sky; and it is at this season, on the southern edge of the belt, that the line of perpetual snow is seen to attain its greatest elevation.

The following are the results of trigonometrical measurements of the elevation of the inferior edge of snow on spurs of the Treslú and Nandádevi groups of peaks, made, before the winter snow had begun, in November 1848.*

Point observed.	Height as observed on face exposed to the East.			Height on face exposed to West observed from Almorah.
	From Almorah (height 5586 ft.).	From Binsar (height 7969 ft.).	Mean.	
No. 1.	16,599 feet.	16,767 feet.	16,683 feet.	15,872 feet.
2.	16,969 ...	17,005 ...	16,987 ...	... ...
3.	17,186 ...	17,185 ...	17,185 ...	14,878 ...
4.	15,293 ...	15,361 ...	15,327 ...	... ...

The points 1, 2, and 3, are in ridges that run from the peaks Nos. 11 and 12 in a south-westerly direction. The dip of the strata being to the north-east, the faces exposed to view from the south are for the most part very abrupt, and snow never accumulates on them to any great extent. This in some measure will account for the height to which the snow is seen to have receded on the eastern exposures, that is, upwards of 17,000 feet. On the western exposures, the ground is less steep, and the snow is seen to have been observed at a considerable less elevation; but it was in very small quantities, and had probably fallen lately, so that I am inclined to think that its height, viz., about 15,000 feet, rather indicates the elevation below which the light autumnal falls of snow were incapable of lying, than that of the inferior edge of the perpetual snow. It is further to be understood, that below this level of 15,000 feet, the mountains were *absolutely without snow*, excepting those small isolated patches that are seen in ravines, or at the head of glaciers, which, of course, do not affect such calculations as these. On the whole, therefore, I consider that the height of the snow-line on the more prominent points of the southern edge of the belt, may be fairly reckoned at 16,000 feet *at the very least*.

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* These measurements make no pretension to accuracy, but are sufficiently good approximations for the purpose for which they are intended. The heights are given as calculated from observations made both at Almorah and Binsar, to shew, in some degree, what confidence may be attached to them. The heights of Almorah and Binsar are on the authority of Captain Webb's survey; the distance of these places, which is used as the base from which to calculate the several distances of the points observed, was got from a map of trigonometrically determined stations obtained from the Surveyor-General's Office.

The point No. 4 was selected as being in a much more retired position than the others. It is situate not far from the head of the Pindur river, and lies between the peaks Nos. 14 and 15. It was quite free from snow at 15,300 feet, and I shall therefore consider 15,000 feet as the elevation of the snow-line in the re-entering angles of the chain.

I conclude, then, that 15,500 feet, the mean of the heights at the most and least prominent points, should be assigned as the mean elevation of the snow-line at the southern limit of the belt of perpetual snow in Kumaon; and I conceive that whatever error there may be in this estimate, will be found to lie on the side of diminution rather than of exaggeration.

This result appears to accord well with what has been observed in the Bissehir range. The account given by Dr Gerard of his visit to the Shátúl Pass, on this range, which he undertook expressly for the purpose of determining the height of the snow-line, contains the only definite information as to the limit of the perpetual snow at the southern edge of the belt, that is to be found in the whole of the published writings of the Gerards; and the following is a short abstract of his observations. Dr Gerard reached the summit of the Shátúl Pass, the elevation of which is 15,500 feet, on the 9th of August 1822, and remained there till the 15th of the same month. He found the southern slope of the range generally free from snow, and he states that it is sometimes left without any whatever. On the top of the pass itself there was no snow; but on the northern slope of the mountain it lay as far down as about 14,000 feet. On his arrival, rain was falling, and out of the four days of his stay on this pass, it either rained or snowed for the greater part of three. The fresh snow that fell during this time did not lie below 16,000 feet, and some of the more precipitous rocks remained clear even up to 17,000 feet.*

The conclusion to which Dr Gerard comes from these facts, is, that the snow-line on the southern face of the Bissehir range is at 15,000 feet above the sea. But I should myself be more inclined, from his account, to consider that 15,500 feet was nearer the truth; and in this view, I am confirmed by verbal accounts of the state of the passes on this range, which I have obtained from persons of my acquaintance, who have crossed them somewhat later in the year. The difference, however, is after all trifling.

Such is the direct evidence that can be offered on the height of the snow-line at the southern limit of the belt of perpetual snow, some additional light may however be thrown on the subject generally, by my shortly explaining the state in which I have found the higher parts of the mountains, at the different seasons during which I have visited them.

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* *Tours in Himalaya*, t. i., pp. 289–347. M. Humboldt apparently interprets Dr Gerard a little too literally, when, with reference to Dr Gerard's statement, that "Hans Bussun," a peak, said to be 17,500 feet high, "had lost all its snow," and looked quite black and dreary," he asks, "Quelle peut étre la cause d'un phénomène local si extraordinaire?" (*Asie Centrale*, t. iii., p. 318, note.) The extreme summit of the peak of Nandádevi, which appears to be a perfect precipice for several thousand feet, is often in much the same predicament of "black and dreary," and many people are disappointed with its appearance for this reason, contrasting it with the beautiful pyramidal peak of No. 19 Pánch-chúli, which is always entirely covered with the purest snow.

In the beginning of May, on the mountains to the east of the Rámganga river, near Námik, I found the ground on the summit of the ridge, called Champwá, not only perfectly free from snow at an elevation of 12,000 feet, but covered with flowers, in some places golden with Caltha and Ranunculus polypetalus, in others purple with primulus. The snow had in fact already receded to upwards of 12,500 feet, beyond which even a few little gentians proclaimed the advent of spring.

Towards the end of the same month, at the head of the Pindur, near the glacier from which that river rises, an open spot on which I could pitch my tent could not be found above 12,000 feet. But here the accumulation of snow, which was considerable in all ravines even below 11,000 feet, is manifestly the result of avalanches and drift. The surface of the glacier, clear ice as well as moraines, was quite free from snow up to nearly 13,000 feet; but the effect of the more retired position of the place in retarding the melting of the snow, was manifest from the less advanced state of the vegetation. During my stay at Pinduri, the weather was very bad, and several inches of snow fell; but excepting where it had fallen on the old snow, it all melted off again in a few hours, even without the assistance of the sun's direct rays. On the glacier at 13,000 feet, it had all disappeared twelve hours after it fell.

On revisiting Pinduri about the middle of October, the change that had taken place was very striking. Now not a sign of snow was to be seen on any part of the road up to the very head of the glacier; a luxuriant vegetation had sprung up, but had already almost entirely perished, and its remains covered the ground as far as I went. From this elevation, about 13,000 feet, evident signs of vegetation could be seen to extend far up the less precipitous mountains. The place is not one at which the height of the perpetual ow car [illegible] estimated, for on all sides are glaciers, and the vast accumulations of snow from which they are supplied, and these cannot always be readily distinguished from snow *in situ;* but as far as I could judge, those places which might be considered as offering a fair criterion, were free from snow up to 15,000 or even 16,000 feet.

Towards the end of August I crossed the Bárjikang pass between Rálam and Juhár, the elevation of which is about 15,300 feet.* There was here no vestige of snow on the ascent to the pass from the south-east, and only a very small patch remained on the north-western face. The view of the continuation of the ridge in a southerly direction was cut off by a prominent point, but no snow lay on that side within 500 feet of the pass, while to the north I estimated that there was no snow in considerable quantity within 1500 feet or more, that is, nearly up to 17,000 feet. The vegetation on the very summit of the pass was far from scanty, though it had already begun to break up into tufts, and had lost that character of continuity which it had maintained to within a height of 500 or 600 feet. Species of Potentilla, Sedum, Saxifraga, Corydalis, Aconitum, Delphinium, Thalictrum, Ranunculus, Saussurea, Gentiana, Pedicularis, Primula, Rheum, and Polygonum, all evidently flourishing in a congenial climate, shewed that the limits of vegetation and region of perpetual snow were still far distant.

In addition to these facts it may not be out of place to mention that there are two mountains visible from Almorah, Rigoli-gúdri in Garhwál between the Kailganga and Nandákni and Chipulá in Kumaon, between the Gori and Dauli (of Dárma), both upwards of 13,000 feet in elevation, from the summits of which the snow disappears long before the end of the summer months, and which do not usually again become covered for the winter till late in December.

The authorities cited by M. Humboldt in his *Asie Centrale* give the following heights to the snow-line on the southern slope of the Himálaya.†

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* This pass is so far within the belt of perpetual snow that it cannot be held to afford any just arguments as to the position of the snow-line on the extreme southern edge of the belt.

† *Asie Centrale,* t. iii., p. 295. I take no account of the height assigned by

	Toises.		English Feet.
Webb,	1954	or	12,500
Colebrooke,	2032	...	13,000
Hodgson,	2110	...	13,500
A. Gerard,	2080	...	13,300
Jacquemont,	1800	...	11,500

Webb, Colebrooke, Hodgson.—Immediately before the list of heights just given, M. Humboldt quotes the following part of a letter from Mr Colebrooke:—"There is a paper of mine in the Journal of the Royal Institution for 1819 (Vol. xvii., No. 13), on the limit of snow. I deduced from the materials which I had, that the limit of constant congelation was 13,000 feet, in the parallel of 31° according to Captain Hodgson's information, and 13,500 feet at lat. 30° according to Captain Webb's."[*] I am unable to refer to the paper here alluded to, but a number of the Quarterly Journal of Science (t. vi., No. 11, pp. 51, 57) has come into my hands, in which is a paper entitled, "Height of the Himalaya Mountains," signed H. T. C., and evidently written by Mr Colebrooke. From this I extract the following sentences:—"The limit of congelation is specified by him (Captain Webb), where he states the elevation of the spot at which the Gori river emerges from the snow, viz., 11,543 feet. This observation, it may be right to remark, is consonant enough to theory, which would assign 11,400 for the boundary of congelation in lat. 30° 25′." Now, as Mr Colebrooke was not an orginal observer, the way in which he talks of the limit of snow and then of the limit of congelation, using them as synonymous terms, would, independently of any other error into which he may have fallen, afford strong grounds for our supposing that he had no very precise ideas as to the meaning of the expression, *limit of snow.* But all doubt on the subject ceases when we learn "that the spot at which the Gori river emerges from the snow" is neither more nor less than the extremity of an immense glacier; and when we see, as I

MM. Hügel and Vigne, as they do not refer to the region to which I confine myself.

[*] The numbers in M. Humboldt's list do no not agree with this; they have possibly been transposed by accident.

have done, that at an elevation not 150 feet less great, and within a mile of this spot, said to be at the limit of constant congelation, is situated Milam, one of the largest villages in Kumaon, where crops of wheat, barley, buckwheat, and mustard, are regularly ripened every year; and that no snow is to be found in the neighbourhood in August or September, at an elevation of at least 16,000 feet,* or 4500 feet above the spot alluded to; it is evident that M. Colebrooke either used the term *limit of snow* in a sense very different from that now applied to it, or has been left altogether in the dark as to those facts on which alone an opinion of any value could be formed.

I am without any means of discovering whether Captains Webb or Hodgson ever published any distinct opinions as to the height of the snow-line, but it appears probable that the information to which M. Colebrooke alludes is simply their record of the heights of places. At all events, however, their evidence must be considered of little value, as they neither of them knew what a glacier was. Captain Webb, as we have seen, talks of the Gori emerging from the snow, when we know that in reality it rises from a glacier. Captain Hodgson falls into a similar error in his description of the source of the Ganges (Vide *Asiatic Researches*, vol. xiv., pp. 114–117). He says "the Bhagirati or Ganges issues from under a very low arch at the foot of the grand snow-bed;" and from the almost exact coincidence of the heights, it is plain that this is his limit of snow. There is not, however, the slightest doubt that the low arch was merely the terminal cave of a glacier, and that it was far below the lower limit of perpetual snow, though when Captain Hodgson was there in the spring the place was probably snowy enough.

A. Gerard.—I have not the means of reference to the passage quoted by M. Humboldt in support of the height given by Captain Gerard; but in the "Account of Koonawur," which may be presumed to shew Captain Gerard's latest views on these matters, he says:—"The limit of perpetual snow is lowest on the outer Himalaya," (by which he means the Bissehir range); "and here the continuous snow-beds exposed to the south are about 15,000.* It is not impossible that the height which M. Humboldt gives refers to some line of perpetual congelation on a number of different varieties, of which Captain Gerard remarks, such as where it always freezes, freezes more than it thaws, freezes every night, or finally, where the mean temperature is 32° Fahrenheit. These, however interesting in their own way, are not the *snow-line.*

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* I say 16,000 feet, as up to that height I am certain; but 18,000 is more probably the truth.

Jacquemont.—The height given by this traveller is fully explained by the note that M. Humboldt adds, "Au nord de Cursali et de Jumnautri ou la limite des neiges est horizontalement très tranchée." (*Jacq.*, *Voy. dans l'Inde*, p. 99.) Now M. Jacquemont visited Jamnotri in the middle of May, when no doubt he found the snow-line, "très tranchée," at 11,500 feet. I have already shewn that I found the same thing myself at Pinduri, where the snow in the autumn had all disappeared up to 15,000 feet or more. If his visit had been made in January, he would probably have found the snow below 8000 feet; but this is not *perpetual snow.*

These heights, therefore, must all be rejected; nor can it be considered at all surprising that any amount of mistake, as to the height of the snow-line, should be made, as long as travellers cannot distinguish snow from glacier ice, or look for the boundary of perpetual snow at the beginning of the spring.

2. *Northern limit of the belt of perpetual snow.*—My own observations on the snow-line in the northern part of the chain were made in September 1848, on my way from Milam

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* *Account of Koonawar*, p. 159. It appears to me possible that the Gerards, who knew as little of glaciers as Webb or Hodgson, did not fall into a similar mistake in their estimate of the height of the snow-line on the Bissehir range, because there are no glaciers, or none of any size, on that face, owing to the small height, less than 2000 feet, that the average line of summit rises above the snow-line. This, however, is only conjecture, for though I am satisfied that glaciers do exist on the north face of that range, I have in vain endeavoured to come to any conclusion as to the southern face. It may be proper to add that I have never been there myself.

into Hundes *viâ* Unta-dhúra, Kyungar-ghát, and Balch-dhúṛa, at the beginning of the month; and on the road back again, *viâ* Lakhur-ghát, at the end of the month.

Of the three passes that we crossed on our way from Milam, all of them being about 17,700 feet in elevation, the first is Unta-dhúra, and we saw no snow on any part of the way up to its very top, which we reached about 4 P.M., in a very disagreeable drizzle of rain and snow. The final ascent to the pass from the south is about 1000 feet; it is very steep at the bottom, and covered with fragments of black slaty limestone. The path leads up the side of a ravine, down which a small stream trickles, the ground having a generally even and rounded surface. Neither on any part of this, nor on the summit of the pass itself, which is tolerably level, were there any remains of snow whatever; the ground being worked up into deep black mud by the feet of the cattle that had been lately returning to Milam. On the ridge to the right and left there were patches of snow a few hundred feet above; and on the northern face of the pass an accumulation remained that extended about 200 feet down, apparently the effect of the drift through the gap in which the pass lies. Below this again the ground was everywhere quite free from snow. On the ascent to Unta-dhúra, at, perhaps, 17,000 feet, a few blades of grass were seen; but, on the whole, it may said to have been utterly devoid of vegetation. On the north side of the pass, 300 or 400 feet below the summit, a Cruciferous plant was the first that was met with.

The Kyungar pass, which is five or six miles north of Unta-dhúra, was found equally free from snow on its southern face and summit, which latter is particularly open and level. The mountains on either side were also free from snow to some height; but on the north, as at Unta-dhúra, a large bed lay a little way down the slope, and extended to about 500 feet from the top. On this pass a Boragineous plant in flower was found above 17,000 feet; a species of *Urtica* was also got about the same altitude, and we afterwards saw it again nearly as high up on the Lákhur pass.

From the Kyungar-ghát, a considerable portion of the southern face of the Balch range, distant about ten miles.

was distinctly seen, apparently quite free from snow. In our ascent to the Balch pass no snow was observed on any of the southern spurs of the range, and only one or two very small patches could be seen from the summit on the north side. The average height of the top of this range can hardly be more than 500 feet greater than that of the pass; and as a whole it certainly does not enter the region of perpetual snow. As viewed from the plains of Hundes, it cannot be said to appear snowy, a few only of the peaks being tipped.

We returned to Milam *viâ* Chirchun. The whole of the ascent to the Lákhur pass was perfectly free from snow to the very top, *i.e.*, 18,300 feet, and many of the neighbouring mountains were bare still higher. The next ridge on this route is Jainti-dhúra, which is passed at an elevation of 18,500 feet, but still without crossing the least portion of snow. The line of perpetual snow is, however, evidently near; for though the Jainti ridge was quite free, and some of the peaks near us were clear probably to upwards of 19,000 feet, yet in more sheltered situations unbroken snow could be seen considerably below us, and, on the whole, I think that 18,500 feet must be nearly the average height of the snow-line at this place.

M. Humboldt's list of heights for the northern slope is as follows:—

	Toises.		English feet.
Webb, ...	2600	or	16,600
Moorcroft, ...	2900	...	18,500
A. Gerard, ...	3200	...	20,500
Jacquemont, ...	3078	...	19,700

Webb.—The height given on the authority of Captain Webb is simply that of the Niti pass, which Captain Webb crossed without snow in August 1819, and Moorcroft in June* and August 1811. The Niti pass is notoriously the easiest of all the Garhwál and Kumaon passes, and remains open long after those from Juhár, which I have described above, have become impracticable; and it is held to be a certain way of escape from Thibet, by the Juháris, should a fall of snow more early than usual stop their own passes, while they are to the north of the Himalaya. It may, therefore, be fairly concluded, that the snow-line recedes considerably above the Niti pass, as it should do if my estimate of its height be correct.

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* Not *January*, as is erroneously printed in the "Asie Centrale." Vide *Asiatic Researches*, vol. xii., pp. 417–494.

Moorcroft.—The passage quoted in support of this height is as follows:—" Now Mr Moorcroft had his tent covered 2 inches deep (with snow), when close to Manasarowar, and on the surface of the ground it lay in greater quantities; and if his elevation was 17,000 feet,* we have clear evidence that the climate of the table-land, notwithstanding the increased heat from the reverberation of a bright sun, is equally as cold as in the regions of eternal snow in the Himalayan chain, although the country of the former exhibits no perpetual snow except at heights of 18,000 and 19,000 feet."—(*Tours in the Himalaya*, t. i., p. 319.) The words are those of Dr Gerard, who, on his own authority, thus gives 18,000 or 19,000 feet as the elevation of the snow-line in the part of Thibet near the Sutlej; and this, as far as it goes, corroborates the conclusion to which I have come.

A. Gerard.—In the absence of the books to which M. Humboldt refers, I conclude that the height here given is that to which Captain Gerard supposed the snow receded on the ridge above Nako. But this is to the north of the Sutlej, and therefore is not in the region to which I have confined myself. In the " Account of Kunawar," however, the following remark that is applicable, is to be found:—" In ascending the Keoobrung pass, 18,313 feet high, in July, no snow was found on the road."—(P. 159.) This pass is situated on the water-shed of the Himalaya, about 20 miles east of the great bend in the Sutlej, and about 8 miles to the south of that river; it is on the northern limit of the belt of perpetual snow, the ground between it and the Sutlej not being of sufficient height to be permanently covered with snow.

Jacquemont.—The Keoobrung pass of Captain Gerard, under a name slightly changed, is the same as that from which

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* The elevation of Manasarowar, as M. Humboldt correctly conjectured, is about 15,200 feet only.

M. Jacquemont made his observations, "Sur le col de Kioubrong (entre les rivières de Buspa et de Shipke ou de Lang Zing Khampa), à 5581 mètres (18,313 feet) de hauteur selon le Capitaine Gerard, je me trouvai encore de beaucoup audessous de la limite des neiges perpétuelles dans cette partie de l'Himalaya (lat. 31° 35′, long. 76° 38′)." "Je crois pouvoir porter la hauteur des neiges permanentes dans cette region de l'Himalaya à 6000 mètres" (19,700 feet).—(*Asie Centrale*, t. iii., p. 304.) I will admit that M. Jacquemont's estimate of the height of the snow-line on the southern face of the range, is not such as to induce me to place implicit confidence in this either; but allowing for some little exaggeration, there can be no room for doubting that the snow-line must here recede nearly to 19,000 feet.

Whether the result at which I have arrived, from what I saw on the Juhár passes, be too little, or this too great, or whether there may not be, in fact, a difference of elevation, are matters of comparatively small importance. As I purpose to point out hereafter, the chances of error in the determination of great altitudes by single barometrical observations are very considerable, more particularly when, as is most generally the case, there is no corresponding observation within 60 or 70 miles. All of these heights are deduced from such observations, and errors of 150, or even 200 feet, on either side of the truth, or differences of 300 or 400 feet, may, I am satisfied, quite easily arise in the calculation. I shall therefore continue to call the height of the snow-line at the northern limit of the belt of perpetual snow 18,500 feet; not that I consider my own calculation as worthy of more confidence than Captain Gerard's or M. Jacquemont's, but that it is, in the present state of our knowledge, sufficiently exact, and certainly not exaggerated.

As the principal object of the present inquiry is the elevation of the snow-line in the Himalaya, I have, in the foregoing observations, confined myself strictly to that region of these mountains that I at first specified; but it is not the less important to notice the heights at which we find perpetual snow still farther to the north. Captain Gerard, after mentioning the Keoobrung pass, goes on to say, "In August when I crossed Manerung pass, 18,612 feet,—a pass on the range that divides Piti from Kunáwar,—" there was only about a foot of snow, which was new, and had fallen a few days before. In October, on the ridge above Nako,"—about five miles north of the great bend in the Sutlej,—" we ascended to 19,411 feet, and the snow, which was all new, and no more than a few inches deep, was only met with in the last 400 or 500 feet; this was on the face of the range exposed to the west, but on the opposite side no snow was seen, at almost 20,000 feet." (P. 160.) During the whole of our expedition into Hundes in September 1848, we only saw very small patches of snow in two places, on both occasions in sheltered ravines; but, in the part of the country through which we passed, perpetual snow is not to be looked for, the highest mountains probably not exceeding 18,000 feet in height. In the true plains of Thibet, snow would be just as difficult to find in the summer months as in the plains of India. From my own observations made in this journey, I infer that the height of the limit of snow, on the southern face of Kailas, is not less than 19,500 feet; and there is nothing now on record that I know of that indicates the latitude beyond which the snow-line again begins to descend.

From a review of the whole of the facts that have been brought forward, it may, I think, be considered as fully established, that M. Humboldt, though under-estimating the actual elevation of the snow-line, was certainly right in what he advanced as to the relative height on the two opposite faces of the chain. The doubts that were raised by Captain Hutton on this point, in his paper entitled, " Correction of the erroneous doctrine, that the snow lies longer and deeper on the southern than on the northern aspect of the Himalaya," were perhaps almost sufficiently answered by Mr Batton at the time they were first brought forward; but, as I have reopened the whole question, I will add a few words on this subject also.*

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* Vide *M'Clelland's Journal*, Nos. xiv., xvi., xix., xxi. Captain Hutton's first letter begins thus: " Previous to my trip through Kunáwar in 1838, I had frequently heard it contended, that the snow lay longer, deeper, and farther

The doctrine that Captain Hutton attacks as erroneous undoubtedly is so; but it is a doctrine that was never inculcated by any one. Captain Hutton having misunderstood the true enunciation of a proposition, reproduces it according to his own mistaken views, and then destroys the phantom that he has raised. The fact that Captain Hutton saw to be true was this, that, as a general rule, snow, sporadic as well as perpetual, will be found to lie at a lower level on the northern than on the southern aspect, on any individual range in these or any other mountains. In drawing his conclusions from this fact, the first error into which he fell was to confound the north and south aspects of the *individual ridges* with the north and south aspects of *the chain;* and he somewhat complicates matters by neglecting to distinguish between *snow* and *perpetual snow.* These mistakes having been pointed out to him, he tried to correct them, but still could not get over the terms *north* and *south declivity*; for he ends by assuming that they apply to the north and south aspects of the Bissehir range, which he conceives to be the true "Himalaya," the central or main line of snowy peaks! Here he falls into an error of logic no less flagrant than the former; he restricts the term "Himalaya" to this range, which may or may not be central, for that has nothing to do with the matter, and then assumes that this Himalaya of his own, is the Himalaya of whose north and south declivities we speak, when we repeat that the snow-line is at a greater

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down on the southern exposure of the Himalaya than it was found to do on the northern aspect; you may, therefore, easily imagine my astonishment, when, crossing the higher passes through Kunâwar, Hungrung, and Pitti, I found the actual phenomena to be diametrically opposite to such a doctrine, and that the northern slopes invariably carried more snow than the southern exposure." (No. xiv., 275.) In his last letter he says, "I have already acknowledged the faultiness of my first letter, in so far as regards my having omitted to state, in sufficiently distinct terms, that my remarks referred to the actual northern and southern aspects of the true Himalaya, or central or main range of snowy peaks, and not to the aspects of secondary groups and minor ranges." This true Himalaya is the Bissehir range of which I have often spoken. I say nothing of Captain Hutton's views regarding perpetual snow, the existence of *which*, as far as I can understand him, he appears to doubt.

elevation on the northern than on the southern face of the chain.*

The height to which the snow-line has been shewn to recede on the southern face of the Himalaya, though considerably greater than had been supposed by M. Humboldt, still does not exceed what the analogy of mountains in similar latitudes in the other hemisphere might have led us to expect. In the Central part of Chili, in lat. 33° S., we find that the lower limit of perpetual snow is at 14,500 or 15,000 feet, while in Bolivia, in lat. 18° S., it reaches 16,000, and even on some of the peaks 19,600 feet.† There is therefore no appearance of any thing unusual in the general height of the snow-line, which need induce us to suppose the existence of any extraordinary ascending current of heated air, regarding which M. Humboldt enquires. The exceedingly high temperature, surpassing that known at any other part of the earth's surface, which the air over the plains of North Western India acquires during the summer, must of course produce a sensible effect in heating the upper strata of the atmosphere. But as far as I am enabled to form an opinion from the few facts that have come to my knowledge, regarding the temperature of the higher regions in these mountains, I think there is little doubt that the same cause which produces this great temperature in the plain, that is, the direct radiation of the sun, acts immediately so powerfully in heating the surface of the mountains, and thereby raising the temperature of the air over them, and in melting the snow, that the secondary effects of the heated air that rises from the plains of India must be almost imperceptible.

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* The word "Himalaya," which, to the natives of these mountains, means only the snowy peaks, is in the language of science applied to the whole chain, and in my opinion properly. Any division of the chain into "Himalaya" or snowy ranges, and "sub-Himalaya" ranges not snowy, such as has, I believe, been made, appears to me objectionable, not only as unusual in the terminology of physical geography, and therefore likely to lead to confusion, such as that of which we have just had a specimen, but as artificial and unnecessary; I repeat artificial, for, in spite of the specious appearance of the distinction, it will not bear examination. The association of *mountains* into *chains* should be based upon the physical character and affinities of the mountains themselves, quite irrespective of any adventitious circumstances of snow, or of vegetable and animal life. Botanical or zoological regions will almost always be found to follow closely the configurations of the earth's surface, on the accidents of which they chiefly depend; but to make the classification of the latter depend upon the former would be a manifest absurdity.

† Asie Centrale, T. iii., pp. 275, 277, 329.

From the way in which the term *north declivity* was introduced into the enunciation of the phenomenon of the greater elevation of the snow-line, at the northern edge of the belt of perpetual snow, an idea naturally arose, that it was observed only on the declivity immediately facing the plains of Thibet, and M. Humboldt, in the quotation I before gave from *Cosmos*, is careful to restrict it to the peaks which rise above the Tartarian plateau. But this, as may have been inferred from what I have already said on the state of the three ranges that are crossed in succession between Milam and Thibet, is quite a mistake ; the fact being that the greater elevation is observed on the Thibetan face in common with the whole of the more northern part of the chain. From the remarks before made on the state in which I found the Barj-Kang pass, it will be seen that even so near as it is to the southern limit of the belt of perpetual snow, a perceptible increase of elevation had already taken place. M. Jacquemont, as quoted by M. Humboldt, says " Les neiges perpetuelles descendent plus bas sur la pente mèridionale de l'Himalaya, que sur les pentes septentrionales, et leur limite s'élève constamment à mesure que l'on s'éloigne vers le nord de la chaine qui borde l'Inde." (*Asie Centrale*, t. iii., p. 303.) With the proviso that the rise here spoken of is not regular, but more rapid as we cross the first great masses of perpetual snow, I entirely concur in M. Jacquemont's way of putting the case.

That the radiation from the *plains of Thibet* can have nothing to do with the greater height to which the snow-line recedes *generally* in the northern part of the Himalaya, is evident, for it must be all intercepted by the outer face of the chain ; and that its effects even on this outer face are of a secondary order, seems to me sufficiently proved by the consideration, that on the Balch range, which rises immediately from those plains, what little snow is to be seen is on the northern slope exposed to the radiation, while none whatever remains on the southern slope, which is quite protected from it, exactly as is the case with every mountain anywhere.

It may therefore be concluded that some other influence must be in operation, the effects of which are generally felt over the whole of the more northern parts of the Himalaya, and such an influence is, I conceive, readily to be found in the diminished quantity of snow that falls on the northern, as compared to the southern part of the chain.

The comparative dryness of the climate to the north of the first great mass of snowy mountains is not now noticed for the first time; it is indeed notorious to the inhabitants of Simla, and travellers often go into Kunáwar with the express object of avoiding the rains. Captain Gerard thus describes the climate of the western part of the Himalaya:—" In the interior (*i. e.* of Kunáwar), at 9000 and 10,000 feet, snow is scarcely ever above a foot in depth, and at 12,000 it is very rarely two feet, although nearer the outer range four or five feet are usual at heights of 7000 or 8000 feet. In these last places there is rain in July, August, and September, but it is not near so heavy in the lower hills. When Hindustan is deluged for three months, the upper parts of Kunáwar are refreshed by partial showers; and, with the exception of the valley of the Buspa, the periodical rains do not extend further to the eastward than long. 77°."*—(*Account of Kunáwar*, p. 61.) He again says, relative to the most northern parts of Kunáwar and the neighbouring portion of Thibet, " With the exception of March and April, in which months there are a few showers, the uniform reports of the inhabitants represent the rest of the year to be almost perpetual sunshine, the few clouds hang about the highest mountains, and a heavy fall of snow or rain is almost unknown."—(*Ibid.*, p. 95.)

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* That the fall of snow at 7000 feet is ever five feet in any part of these hills may, I think, be doubted. The Buspa is the river that runs immediately at the foot of the north declivity of the Bissehir range; and I suppose that Captain Gerard means, that the rains do not extend up the Sutlej beyond the point where the Buspa falls into it.

The testimony of Captain J. Cunningham, who passed a winter in the most northern part of Kunáwar, as to the small quantity of snow that falls, is particularly valuable. He says, "In this country a southerly wind and the sun together, keep slopes with a southern exposure and 12,000 and 13,000 feet high, quite clear of snow, (except when it is actually snowing.) And this too, towards the end of January and beginning of February, or I may say at all times." Also, "Here I am (April 6th 1842,) about 9000 or 9500 feet high, wind generally southerly, no snow whatever on southern slopes, within 15,000 or 16,000 feet, apricot trees budding; but on northern slopes, and in hollows, abundance of snow."* (*M'Clelland's Calcutta Journal of Natural History*, No. xiv., pp. 281, 282.)

From my own experience, I can also speak of the remarkable change of climate that is met with in the month of August, in passing from the south to the north of the line of great peaks, by the valleys of the Gori and Rálam rivers. A straight line joining the peaks No. 14 (Nandádevi), and No. 18 (the northern of the Panch-Chuli Cluster), cuts the Gori a little below Tola and the Rálam River, about 5 miles further to the east, near the village of Rálam. The road up the Gori being at that season impracticable, I went up the Rálam river to Rálam, and thence crossed over the Gori by the Bargi-Káng pass, which is on the ridge that separates the two rivers, and that terminates in the peak No. 16 (Hansá-Ung.) From the limit of forest to the village of Rálam, the elevation of which is about 12,000 feet, the vegetation, chiefly herbaceous, was of the most luxuriant growth and boundless variety, and the soil was saturated with moisture. On crossing the Bargi-Káng pass, and descending to the Gori, we were immediately struck with the remarkable change in the character of the vegetation, which had already lost all its rankness. But a mile or two above the village of

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* These paragraphs are taken from extracts of letters of Captain Cunningham, given by Captain Hutton, in support of his arguments, as to snow lying lower on north than on south exposures, which accounts for the last sentence. But whatever the quantity of snow may have been on the north slopes, compare the heights here given as being clear of snow, early in *April*, viz., 15,000 feet, with what I have above shewn to be the limit to the south of the great peaks, as late as the middle of May, viz., 12,500 feet.

Tola, the alteration was complete; the flora had shrunk within the most scanty limits, the bushes hardly ever deserving the name of shrubs; the few herbs that were there were stunted and parched, the soil dry, and the roads quite dusty. At Melam the still closer approximation of the climate to that of Thibet, is clearly shewn by the occurrence of several plants undoubtedly Thibetan, that are not found further to the south. Such are Caragana versicolor, the (Dama) of the Bhotias, which covers the plains of Thibet; a Clematis, dwarf Hippophaë, Lonicera, and two or three Potentillas; and no doubt several others might be named.

Now although it is to the winter and not to the summer rains,* that the precipitation of snow on these mountains is to be ascribed, yet the circumstances under which the vapour is condensed, appeared to be the same at both seasons. Southerly winds blow throughout the year over the Himalaya, in the winter with peculiar violence;† and whatever be the more remote cause of the periodical recurrence of the rains, there can I think be little doubt, that the proximate cause of the condensation of by far the greater portion of the snow or rain that falls on the snowy mountains, is that the current from the south is more damp or hot than the air in contact with the mountains against which it blows; a relation which holds good in the winter as well as in the summer.

Thus the air that comes up from the south no sooner reaches the southern boundary of the belt of perpetual snow, where the mountains suddenly rise from an average of perhaps 8,000 or 10,000 feet, to nearly 19,000 or 20,000, then it is deprived of a very large proportion of its moisture, which is converted into cloud, rain, or snow, according to circumstances. And the current, in its progress to the north, will be incapable of carrying with it more moisture, than is allowed by the very low temperature to which the air is of necessity reduced in surmounting the snowy barrier, 19,000 or 20,000 feet in altitude, that it has to pass. Nor can any further condensation be expected at all comparable in amount to what has already taken place, as it would manifestly demand a much more than corresponding depression of temperature; and this is not at all likely to occur, for the most elevated peaks being situated near the southern limit of perpetual snow, the current on passing them will more probably meet with hotter than with colder air.

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* Although it does not appear to be so well known, the winter rains of North Western India are as strictly periodical as those of the summer.

† The southerly winds that prevail at considerable heights in the Himalaya, and in the countries to the north, are diurnal phenomena, evidently dependent on the apparent motion of the sun; and in their time of beginning of maximum and of ending, greatly resemble the hot winds of the plains of India, which have a similar origin.

It is, I conceive, to precisely similar causes that we should attribute the great amount of rain that is known to fall at Mahabaleshwar, on the Western Ghats, at Chira-punji, in Sylhet, and generally, though the quantity is far less, along the most southern range of the Himalaya itself; and it is curious to observe that the comparative dryness of the less elevated country to leeward also holds good in these cases. In the Deccan, the country immediately to the east of the Western Ghats, Colonel Sykes tells us, that "the rains are light, uncertain, and in all years barely sufficient for the wants of the husbandman." On the same authority we find, that while the mean fall of rain for three years at Poona, was about 27 inches,* that at Mahabaleshwar for 1834 was no less than 302 inches.† Although I have not the exact figures to refer to, I know that the rain at Nainee Tal, on the external range of the Himalaya, is about double what falls at Almorah, not thirty miles to the north.

It will therefore be seen that as I hold the direct action of the sun to be the primary cause of the great general height to which the snow-line recedes, so I consider that the increase of the height in the northern part of the chain, chiefly depends, not on any additional destructive action, but on the smaller resistance offered by a diminished quantity of snow to destructive forces, which are not indeed constant throughout the whole breadth of the chain, but whose increase appears to have no dependence on increase of distance from the southern limit of the belt of perpetual snow. Among the more evident causes of the irregularities in the melting of the snow, may be mentioned, the powerful action of the heavy summer rain on the southern face, as compared with what falls as little more than a drizzle on the northern; the protection afforded from the radiation of the sun by the heavy clouds so frequent in the south, contrasted with the relative slight resistance of the less dense but not uncommon clouds on the north; the differences in the temperature of the air that acts on the lower edge of the snow produced by the difference of height of the snow-line on the opposite faces of the chain; and, lastly, the differences of the temperature of the air, and of the amount of radiation and reflection dependent on the differences in the state of the surface of the earth, which on the south is densely clothed with vegetation, while on the north it is almost bare.

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* British Association's Seventh Report, p. 236.

† Ibid., Ninth Report, p. 15 (Sections). The exact amount is 302·21 inches.

Before concluding I will observe, that the height at which it is certain that snow will fall every year, in this region of the Himalaya, is about 6500 feet; and at an elevation of 5000 feet it will not fail more than one year out of ten. The least height to which sporadic falls of snow are known to extend, is about 2500 feet; and of such falls there are only two authentic instances on record, since the British took possession of Kumaon, viz., in 1817 and 1847. Thus we see that the regular annual fluctuation of the snow-line is from 9000 feet to 10,500 feet, and it occasionally reaches even 13,000 feet. M. Humboldt informs us that under the equator at Quito, the fluctuation is 600 toises (3800); that at Mexico it reaches 1350 toises (8600 feet); and the greatest fluctuation that he mentions is that in the south of Spain, which amounts to 1700 toises (10,900).*

A brief recapitulation of the principal results of this inquiry will shew us, that the snow-line, or the southern edge of the belt of perpetual snow in this portion of the Himalaya, is at an elevation of 15,500 feet, while on the northern edge it reaches 18,500 feet; and that on the mountains to the north of the Sutlej, or still farther, recedes even beyond 19,000 feet. The greater elevation which the snow-line attains on the northern edge of the belt of perpetual snow, is a phenomenon not confined to the Thibetan declivity alone, but extending far into the interior of the chain ; and it appears to be chiefly caused by the quantity of snow that falls on the northern portion of the mountains being much less than that which falls further to the south, along the line where the peaks, covered with perpetual snow, first rise above the less elevated ranges of the Himalaya.—(*Journal of the Asiatic Society of Bengal.* New Series. No. xxviii., p. 287.)

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* Asie Centrale, t. iii. p. 279.

On Comparative Physical Geography.

It may interest our readers to be informed that *Physical Geography*, founded on the views of Ritter, Humboldt, Steffens, &c., has been explained and illustrated in an interesting course of lectures delivered at Boston, in North America, by a distinguished Swiss naturalist, Professor Guyot, formerly of Neufchatel, now resident in the New World. Professor Felton, of Harvard University, U. S., has published, under the superintendence of M. Guyot, an English version of these lectures (the Lectures were delivered in the French language), under the title, " The Earth and Man: Lectures on Comparative Physical Geography, in its relation to the History of Mankind." Men of science speak of them in high terms. Thus the celebrated Agassiz says of Guyot, " He has not only been in the best school, that of Ritter and Humboldt, and become familiar with the present state of the science of our earth, but he has himself, in many instances, drawn new conclusions from the facts now ascertained, and presented most of them in a new point of view. Several of the most brilliant generalizations developed in his lectures are his; and if more extensively circulated, will not only render the study of geography more attractive, but actually shew it in its true light; namely, as the science of the relations which exist between nature and man throughout history; of the contrasts observed between the different parts of the globe; of the laws of horizontal and vertical forms of the dry land, in its contact with the sea; of climate, &c.;" and Professor Felton remarks, " That although physical science in general lies beyond his sphere of studies, he may venture to express the opinion, that physical geography, as treated of late years by Humboldt, Ritter, and other European investigators, has risen to a rank of paramount importance, in its bearing upon the history and the destinies of the human race. It is not too much, perhaps, to say, that the history of man cannot be properly understood unless it rest on the basis of this science; and to come nearer to my own pursuits, I know that comparative philology, especially in connection with the affinities of the different branches of the family of man, receives important light from the great conclusions of physical geography. The physical characteristics of our globe, and their influences upon human societies, are described in these Lectures with vivacity and elegance. The contrasts between the different portions of the earth, their reactions upon each other, their adaptation to the special part that each, in the order of Providence, has been called upon to perform in the drama of human history, are given in a most interesting manner. It cannot escape the attention of the readers of these Lectures, how constantly the relations of the earth to the Creator—the reference of all things to the designs of Infinite Goodness, Wisdom, and Power—and how earnestly the moral and religious lessons drawn from a profound conviction of the truth of Christianity, are brought forward and enforced by Professor Guyot, as forming the great central and binding facts which give a living energy to the system of nature, and explain the course of the world." As Guyot's work is scarcely known in Britain, we embrace this opportunity of laying before our readers one of the Lectures (the 11th Lecture), as a specimen of the manner in which the Swiss naturalist treats his subject.

The continents of the North considered as the theatre of History; Asia-Europe; contrast of the North and South; its influence in history; conflict of the barbarous nations of the North with the civilized nations of the South; contrast of the East and West; Eastern Asia a continent by itself, and complete; its nature; the Mongolian Race belongs peculiarly to it; character of its civilization; superiority of the Hindoo Civilization; reason why these Nations have remained stationary; Western Asia and Europe; the country of the truly historical races; Western Asia; physical description; its historical character; Europe—the best organized for the development of man and of societies; America—future to which it is destined by its physical nature.

The result of the comparison which we have made between the northern continents and the southern continents,* in their most general characteristics, has convinced us, if I do not deceive myself, that what distinguishes the former is not the wealth of nature and the abundance of physical life, but the aptitude which their structure, their situation, and their climate, give them, to minister to the development of man, and to become thus the seat of a life much superior to that of nature. The three continents of the north, with their more perfect races, their civilized people, have appeared as the *historical* continents, which form a marked contrast to those of the south, with their inferior races and their savage tribes.

Since this is the salient feature which distinguishes them, and which secures to them decidedly the first place, we shall proceed to study them more in detail as the theatre of history.

We know beforehand, that the condition of an active, complete development, is the multiplicity of the contrasts, of the differences—springs of action and reaction, of mutual exchanges, which excite and manifest life under a thousand diverse forms. To this principle corresponds, in the organization of the animal, the greater number of its special organs; in the continents, the variety of the plastic forms of the soil, the localization of the strongly characterised physical districts, the nature of which stamps upon the people inhabiting them a special seal, and makes them so many complicated but distinct individuals.

The various combinations of grouping, of situation, with regard to each other, placing them in a permanent relation of friendship or hostility, of sympathy or of antipathy, of peace or of war, of interchange of religions, of manners, of civilization, complete this work, and give that impulse, that progressive movement, which is the trait whereby the historical nations are recognised.

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* The northern continents are Europe, Asia, and North America; the southern continents are Africa, Southern Asia, and South America.

We may then expect to see the great facts of the life of the nations connect themselves essentially with these differences of soil and climate, with these contrasts that nature herself presents in the interior of the continents, and whose influence on the social development of man, although variable according to the times, is no less evident in all the periods of his history.

Let us commence our inquiry with the true theatre of history—with Asia-Europe.

We have already had occasion to call attention to the unity of plan exhibited in this great triangular mass, which authorises us to consider it as forming, in a natural point of view, a single continent, the subdivisions of which bear the imprint of only secondary differences. We have also indicated, as the most remarkable trait of its structure, that great dorsal ridge, composed of systems of the loftiest mountains, traversing it from one end to the other in the direction of the length, which may even be regarded as the axis of the continent. It is, in fact, on the two sides of this long line of more than 5000 miles, on the north and south of the Himalaya, of the Caucasus, of the Balkan, the Alps, and the Pyrenees, that the high lands of the interior of the continent extend. It splits Asia-Europe into two portions, unequal in size, and differing from each other in their configuration and their climate. On the south, the areas are less vast; the lands are more indented, more detached,—on the whole, perhaps, more elevated; it is the maritime zone of peninsulas. On the north, the great plains prevail; the peninsulas are rare, or of slight importance; the ground less varied.

But what chiefly distinguishes one of the two parts from the other, what gives to each a peculiar nature, is the climate. Those lofty barriers which we have just named, almost everywhere separate the climates, as well as the areas. The gradual elevation of the terraces towards the south, up to this ridge of the continent, by prolonging in the southern direction the frosts of the north, augments still further, in Eastern Asia and in Europe, the difference of temperature between their sides, and renders it more sensible. Thus, almost everywhere, the transition is abrupt, the two natures wide apart. These high ridges arrest at once the icy winds of the poles, and the softened breezes of the south, and separate their domains. The Italian of our days, like the Roman of former times, boasts of his blue sky and his mild climate, and speaks with an ill-concealed contempt of the frosts and the ice of the countries beyond the Alps.

To the father of the Grecian poets, to Homer, who only knows the Ionian sky, the countries beyond the Hæmus are the regions of darkness, where rugged Boreas reigns supreme. At the northern foot of the Caucasus, the dry steppes of the Manytsch are swept by the frozen winds of the north; on the south, the warm and fertile plains of Georgia and of Imereth feel no longer their assaults. In eastern Asia, finally, the contrast is pushed to an extreme. The traveller, crossing the lofty chain of the Himalaya, passes suddenly from the polar climate of the high table-lands of Thibet to the tropical heats and the rich nature of the plains of the Indus and the Ganges. Yet, as we have said, this great wall, which separates the north from the south, is rent at several points. Between the Hindoo-Khu and the Caucasus, the depressed edge of the table-land of Khorasan, between the Caucasus and the Balkan, the plains of the Black Sea and of the Danube open wide their gates to the winds and to the nations of the shores of the Caspian and the Volga. Between the Pyrenees and the Alps, the climates and the people of the south penetrate into the north.

Thus two opposite regions are confronted, one on the north, in the cool temperate zone, with its vast steppes and desert table-lands, its rigorous climates, its intense colds, its dry and starveling nature; the other on the south, in the warm temperate zone, with its beautiful peninsulas, its fertile plains, its blue heavens, and its soft climate, its delicate fruits, its trees always green, its lovely and smiling nature.

The contrast of these two natures cannot fail to have a great influence on the people of the two regions. It is repeated, from the history of the very earliest ages, in the most remarkable manner. In the north, the arid table-lands, the steppes, and the forests, condemn man to the life of shepherds and hunters; the people are nomadic and barbarous. In the south, the fruitful plains, and a more facile nature, invite the people to agriculture; they form fixed establishments and become civilized. Thus, in the very interior of the historical continent we find, placed side by side, a civilized and a barbarous world.

Two worlds so different cannot remain in contact without reacting upon each other. The conflict begins, one might say, with history itself, and continues throughout its entire duration. There is scarcely one of the great evolutions, particularly in Asia, not connected with this incessant action and reaction of the north upon the south, and of the south upon the north, of the barbarian world upon the civilized world. At all periods we see torrents of barbarous nations of the north issuing from their borders and flooding the regions of civilization with their destroying waves. Like the boisterous and icy winds of the regions they inhabit, they come suddenly as the tempest, and overturn everything in their way; nothing resists their rage. But as after the storm nature assumes a new strength, so the civilized nations, enervated by too long prosperity, are restored to life and youth by the mixture of these rough but vigorous children of the north. Such is the spectacle presented to us by the history of the great monarchies of Asia and of their dynasties; that of Europe is scarcely less fertile in struggles of this kind. Some examples which I proceed to recal to your memory will be enough to convince you of the powerful influence of this contrast.

As far as the memorials of history ascend, it shews us, on the table-land of Iran, and in the neighbouring plains of Bactriana, one of the earliest civilized nations, the ancient people of Zend. The Zendavesta, the sacred book of their legislator, displays everywhere deep traces of the conflict of Iran, of the southern region, of the light of civilization—*the good*—with the Turan, the countries of the North, the darkness, the barbarous peoples—*the evil*. Who can say that even the idea of this dualism—of good and evil—which is the very foundation of the religious philosophy of Zoroaster, is not, to a certain extent, the result of the hostile relations between two countries so completely different? Six centuries before Christ, the barbarous Scythians come down from the North, pass like a whirlwind through the same gate of the Khorasan upon the plateau of Iran, overrun the flourishing kingdom of Media, and spread themselves as far as Egypt. A whole generation was necessary to restore to Cyaxares his crown, and to efface the traces of this rude attack. In the eleventh century of our era, the Seldjouks—Turks,—descend from the heights of Bolor and Turkestan, invade first Eastern Persia, overturn the power of the Gaznevide Sultans, put an end to that of the Caliphs, and lord it over Western Asia. But nothing equals the tremendous shock caused through the whole of Asia by the invasion of the Mongolians. Issuing from their steppes and their deserts, under the conduct of the daring Gengis-Khan, the hero of his nation, their ferocious hordes spread like a devastating torrent from one end of Asia to another. Nothing withstands their onset; even Europe itself is threatened by these barbarians; all Russia is subjected, and scarcely can the assembled warriors of Germany drive them back from their frontiers, and save the nascent civilization of the West. China herself beholds a succession of conquerors establish in the North a brilliant empire, and for the first time the two Asias are subject to one and the same dominant people. India alone had been spared; she yields before a fresh invasion, and Sultan Babur—who already is no more a barbarian—founds, at the beginning of the sixteenth century, the mighty Mongolian empire, which, in spite of its vicissitudes, has existed down to our days, and has yielded only to the power of the nations of civilized Europe. The history of China, lastly, is crowded with the struggles of the civilized people of the plain with the roving tribes of the neighbouring table-lands, and the last of these invasions, so frequent,—that of the Manchou Tartars,—has given to China its present rulers.

In Europe, the war of the North against the South, though seemingly not so long continued, is not less serious. Six centuries before our era, bands of Celts, enticed by the attractions of the fertile countries of the South, set forth from Gaul, under the lead of Bellovese and Sigovese, cross the Alps, and proceed to establish themselves in the smiling Plains of the Po. Other bands follow them thither, and found a new Gaul beyond the Alps. These impetuous children of the North soon press upon Etruria, and Rome, which has drawn upon herself their anger, suffers the penalty of her rashness. About 390 B.C. the city was burnt, and the future mistress of the world wellnigh perished in her cradle, by the strong hand of the very men of the North whom she was destined afterwards to subject to her laws. A century later, these same Gauls, who find Rome victorious and Italy shut against them, rush upon enervated Greece, give her up to pillage, and, profaning the sacred temple at Delphi, announce the fall of Greece, and the last days of her glory and her liberty. Another troop of these bold adventurers cut their way into Asia Minor; they maintain themselves there, objects of terror in the land which bears their name, to the very moment when the power of Rome forced all the nations to bow beneath her iron yoke.

A century before the birth of our Saviour, the men of the north are again in motion. The Cimbri and the Teutons appear at the gates of Italy, and spread terror even to Rome herself. Forty years have scarce rolled away when Rome, in her turn, assails the Northern world. Cæsar marches to conquer the Gauls, formerly so terrible, and in the course of ages they are won to civilization. Thus, by the third gate which opens the wall of separation, the Southern world penetrates into that of the North.

But a still more earnest struggle then commences. The Germans have preserved their native energy, and are still free. Rome is declining, and, little by little, the sources of life in that immense body are drying up. The weaker it grows, the more the men of the North press upon the mighty colossus, whose head is still of iron, though its feet are of clay. It falls for its own happiness and that of humanity; for a new sap—the fresh vitality of the Northmen—is to circulate through it, and soon shall it be born again, full of strength and life.

You see, from the beginning to the end of history, the contrast of these two natures exercises its mighty influence. The struggle between the people of the two worlds is constant. In Asia it may be again renewed, for nature there is unconquerable, and the contrast still exists. In Europe, the coarse struggle of brute strength of the early days has ended, since, culture having passed into the North, conquerors and conquered, civilized men and barbarians, have melted down into one and the same people, to rise to a civilization far superior to the preceding. But we behold it reappear, less material but not less evident, between the free and intelligent thinker, the Protestant of the North, and the artistic, impassioned, superstitious Catholic man of the South.

Let us now pass to a second feature of the structure of the continent Asia-Europe, which has almost as much weight as that we have just discussed.

Long chains, extending from the North to the South, in the direction of the meridians, the Bolor and Mount Soliman, cut at right angles the great east-west axis. The Bolor forms the western margin of the high central plateau; the Soliman the eastern margin of the table-land of Iran,—the one on the north, the other on the south; so that these two solid masses touch each other at their opposite angles, south-west and north-east. The remarkable point where these high ranges intersect, and the table-land and the plains, spread out at their feet, touch each other, is the Hindo-Khu. These features of relief sever the continent into two parts, of almost equal extent, but of very unequal importance; Eastern Asia on the one side, and Western Asia and Europe on the other,—the Mongolian races and the White races.

This separation is so deeply marked in nature and in the nations, that even the ancients, with the practical sense belonging to them, made a division of Asia *intra Imaum* and Asia *extra Imaum*, that is, Asia this side, and Asia beyond the Bolor and the Hindo-Khu, as they also divided the north and the south into Scythia—Nomadic Asia—and Asia Proper, or civilized Asia.

Eastern Asia forms, in fact, a continent by itself alone. A vast pile of high lands, a plateau in the form of a trapezium, occupies the entire centre, and forms the principal mass. It seems to invade everything; it is the prominent feature, and gives a distinctive physiognomy to the continent. It is surrounded on all sides by lofty ranges capped with snow, which seem, like towering ramparts, to guard it from attack, and to isolate it on every side. On the south the Himalaya, on the west the Bolor, on the north the Altai, on the east the Khin-gan, and the Yun-Ling form an enclosure almost unbroken, the detached summits of which belong to the loftiest mountains of the earth. A small number of natural entrances lead to the interior, or give an exit from it. The only gate which offers some facility is Zungary, between the Thian-Shan and the Altai; everywhere else, high and frozen passes.

The interior of this vast enclosure is cut by numerous chains, the highest of which—those of the Kuenlun on the south, and of the the Thian-Shan on the north—are parallel to the Himalaya and the Altai, and divide the soil into several basins or high bottoms. In all this extent, no fertile and easily cultivated plain; everywhere stretch the steppes, a dry and cold desert, or seas of drifting sand. Nevertheless, a considerable depression in Eastern Turkestan, where the Tarim flows, and whose bottom is marked by Lake Lop, allows the cultivation of the vine and the cotton-tree, at the foot of the Thian-Shan; but this is an exception. Apart from some privileged localities, nature here does not permit a regular tillage, and dooms the tribes of these regions to the life of shepherds and herdsmen,—the nomadic life.

Around this central mass, towards the four winds of heaven, extend, at its feet, broad and low plains, watered by the rivers pouring down from its heights, which rank among the largest in the world. On the north is the most extensive but the least important, the frozen and barren plain of Siberia, with the streams of the Obi, the Jenisey, the Lena; on the east the low country of China, where meet and unite the two giant rivers of the Old World,—those two twin rivers, which, born in the same cradle, flow on to die in the same ocean; on the south the plain of Hindostan, moistened by the fresh and abundant waters of the Himalaya, and the sacred streams of the Indus and the Ganges; on the west, finally, the plain of Turan, with the two rivers of Gihon and Sihon, and its salt seas, to which Western Asia already lays claim. It is in these plains, with fruitful alluvial soil, and on the banks of these blessed rivers, that were developed the earliest, almost the only, civilized nations belonging to this continent. But the warm and maritime region of the east and the south, connected with the rich peninsulas of India, is by far the most favoured of all. China and India, therefore, have given birth to the two great cultivated nations of Eastern Asia.

Nevertheless, as the great central ridge swerves obliquely towards the south, this warm and fortunate region forms only a narrow strip, not to be compared in extent with the cold, and steril, and barbarous world of the North. This predominates, and gives its character.

Such are the distinctive features of Eastern Asia. What strikes us in this world of the remotest East, is its gigantic proportions. The loftiest mountains of the earth, the most massive table-lands, the most extensive plains, peninsulas which are small continents, rivers which have no rivals in the Old World, give to it a character of grandeur and majesty nowhere else to be found. But it is easily understood; nowhere are the differences also so strongly drawn, so huge, so invincible. Nowhere is the contrast between the high lands and the low lands, between the heat and the cold, between the moisture and the dryness, abundance and sterility, presented on so vast a scale. See, by the side of the low, burning, and productive plains of Hindostan, 10,000 or 15,000 feet higher up, the cold and arid high land plain of Thibet and Tangout; by the side of China and its populous cities, the elevated deserts and the tents of the nomades of Mongolia. The differences are everywhere pushed to their utmost limit.

Furthermore—and this characteristic completes the picture—the communications from one region to another are always difficult. One thoroughfare alone, the valley of the Peschawer, leads from Persia to India, and has been the highway of all the conquerors from Alexander to Babur and the English. No practicable road for armies or for regular commerce unites India and China; the peninsulas communicate only by sea. The passes of the Himalaya are at an elevation of from 10,000 to 18,000 feet; those of the Bolor are frozen in the middle of summer. At all times the passage of the plateau is a difficult and tedious undertaking, and at certain points almost impossible.

Eastern Asia is, then, pre-eminently the country of contrasts, of isolated and strongly characterised regions; for each forms a world apart, and is sufficient unto itself.

What must be the effect of this strong and massive nature upon the nations who live under its influence, history will inform us.

As Eastern Asia has a physical nature which belongs especially to itself, so it has a particular race of men, the Mongolian race. We have already pointed out the external characteristics of the Mongolian family. With it the melancholic temperament seems to prevail; the intellect, moderate in range, exercises itself upon the details, but never rises to the general ideas or high speculations of science and philosophy. Ingenious, inventive, full of sagacity for the useful arts and the conveniences of life, the Mongolian, nevertheless, is incompetent to generalize their application. Wholly turned to the things of earth, the world of ideas, the spiritual world, seems closed against him. His whole philosophy and religion are reduced to a code of social morals, limited to the expression of those principles of human conscience, without the observance of which society is impossible.

The principal seat of the Mongolian race is the central table-land of Asia. The roaming life and the patriarchal form of their societies are the necessary consequence of the steril and arid nature of the regions they inhabit. In this social state, the relations and the ties which unite the individuals of the same nation are imposed by kindred, by birth—that is, by nature. Association is compulsive, not of free consent, as in more improved societies. Thus, the greater part of Eastern Asia seems doomed to remain in this inferior state of culture; for the whole North—Siberia and its vast areas—is scarcely more suited to favour the unfolding of a superior nature.

Nevertheless, in the warm and maritime zone, in the fertile and happy plains of China and India, along those rivers which support life and abundance on their banks, nations, invited by so many advantages, establish themselves, and fix their dwelling-places. Their number soon augments; they demand their support from the soil, which an easy tillage yields them in abundance. They become husbandmen; cultivated societies are formed; civilization rises to a height unknown to the tribes of the table-land.

The Chinese, of Mongolian race, preserves, even in his civilization, the character as well as the social principle stamped upon his race by nature,—the patriarchal form. The whole nation is a large family; the Emperor is the father of the family, whose absolute, despotic, but benevolent power governs all things by his will alone. China, then, in the order of civilized nations, is the purest representative of Eastern Asia, and shews us to what point the patriarchal principle of the earliest communities is compatible with a higher cultivation.

In India, the nations of the White race, sprung from the West, have founded a civilization wholly different, the character of which is explained at once by the primitive qualities of the race and the climate.

Endowed with a higher intelligence, with a power of generalization, with a profound religious sentiment, the Hindoo is the opposite of the Chinese. For him the invisible world, unknown by the Chinese, seems alone to exist. But the influence of the climate of the tropics gives to the intuitive faculties an exaggerated preponderance over the active faculties. The real, positive world disappears from his eyes. Thus in his literature, so rich in works of high philosophy, of poetry, and religion, we seek in vain for the annals of his history, or any treatise on science, any of those collections of observations so numerous among the Chinese. In spite of these defects the Hindoo civilization, compared to that of China, bears a character of superiority which betrays its noble origin. It is the civilization of the western races transported and placed under the influence of the East.

But there is one characteristic common to all these civilizations of the uttermost East, which deserves our particular attention. Born in the earliest ages of the world (for without admitting,—far from it,—the fabulous antiquity their own traditions assign them, we may regard them as belonging to the most ancient in the world), they seem to grow rapidly at first; and, at the remotest period recorded by history, they have already acquired the degree of development, and all the leading features which distinguish them at the present day. Nearly 1500 years before Christ,—others say 2000,—India already possessed the Vedas,—those religious and philosophical works, which already suppose a high culture and its accompanying social state. Alexander finds it flourishing and brilliant still, but little changed; the description the historians of his conquests have left, is true of modern India when invaded by the English. As much may be said of China, whose existing condition seems to present the same essential features which we know it to have possessed from a time long before our era. Thus, these nations offer us the astonishing spectacle of civilized communities remaining perfectly stationary. 3000 years of existence have made no essential change in their condition,—have taught them nothing,—have brought about no real progress,—have developed none of those great ideas, which effect, in the life of nations, a complete transformation. They are, as it were, stereotyped.

What, then, has been wanting to these people, that they have not been favoured with a further progress? Why do they all stop short in the career upon which they have entered in so brilliant a manner, —even the Hindoos of noble race,—of the race eminently progressive?

What has been wanting to the communities of Eastern Asia is the possibility of actions and re-actions upon each other, more intimate, more permanent; it is the possibility of a common life.

These nations are too isolated by nature,—too opposite in race and character, to be able to blend in one common civilization. The Hindoos are separated from China by the snowy terraces of the Himalaya and of the Yun-Nan; from Western Asia by the high table-lands of Caboul. These forms of relief are too huge—the contrasts resulting from them are too violent; they are unconquerable by man. Meantime, each of these rich districts may suffice, of itself alone, for a beautiful career of improvement; their excellences, as well as their defects, run into excess; nothing tempers or corrects them; their character is more individual. Such is the strength of these civilizations, that clouds of conquerors are successively absorbed, without modifying them, almost without leaving a trace behind.

But individuality is here carried to egoism. Of this very isolation which causes their inferiority, and which kills all progress, they make a conservative principle. The Hindoo cannot leave his country except by sea: the Vedas forbid it under pain of pollution. Japan and China obstinately close their borders against all the nations of Europe, and it is only at the cannon's mouth that the English have opened the gates so long shut, and forced them to the life of interchange which will restore them to progress and vitality. Thus, while every thing around them is advancing, India and China have remained stationary. For it is not given to one people alone, any more than to one individual alone, to run through the whole compass of the scale of human progress by themselves, and without the aid of their brethren.

Eastern Asia is, then, the continent of extreme contrasts and of isolated regions,—of races essentially Mongolian,—of stationary civilizations,—of the semi-historical nations. It is not there that the work of the development of humanity can be achieved.

The second half of the Old World, in the temperate region, Western Asia and Europe, forms another whole, in which we are able to point out several common characteristics. Besides the division into a North and a South, on the two sides of the continental axis, the most salient feature is the long table-land of Iran, which stretches uninterruptedly from India to the extremity of Asia Minor, and even prolongs itself, without losing its nature, across the peninsulas of the Mediterranean, as far as Spain.

From one end of these regions to the other nature wears a character of uniformity. Everywhere the same cretaceous and jurassic limestone-deposits form the greater part of the ground; everywhere volcanoes rise from the earth, and shake it with their convulsions. The climate, also, is alike; for in Asia a more southern latitude is counterbalanced by a greater elevation of the plateaux. The flora is analogous; the cultivated plants, the fruits, the domestic animals, are the same, with the exception of the camel of the desert, useless to Europe. Finally, the white Caucasian race, the most noble, the most intellectual of the human species, dwells there, and all the nations of progressive civilization. If we add Egypt and the vicinage of the Atlas, which belong to the Mediterranean, it is the true theatre of history, in the proper meaning of that word. Nevertheless, in spite of this real community of characteristics, it is easy to detect, in Western Asia and Europe, certain differences not less important, which force us to consider them still as two distinct continents.

In Europe, in the southern zone, the plateau loses its continuity, and splits into peninsulas. In the northern zone, the arid steppes and the deserts are changed beyond the Ural into a fertile soil, more elevated, well watered, covered with forests, and susceptible of cultivation. The areas become gradually smaller, and the whole continent is only a great peninsula, of which the headland, turning towards the west, juts out into the ocean. The north-east direction of the continental axis, crowding the lands farther north, and the influence of the ocean, give it a wetter and a more temperate climate. Let us further examine these two portions of Asia-Europe considered in the historical point of view. Western Asia is placed in the middle portion of the continent; Asia-Europe between the two extreme parts. Like Eastern Asia it has for its centre and prominent feature a table-land encircled with mountains, the plateau of Iran and of Asia Minor; but it is narrower, more elongated. The mountain-chains are less elevated, less continuous. The mountains of Kurdistan and of the Taurus, which edge it on the south, attain a height of 10,000 or 12,000 feet only at a few points. The higher mountains, as the Ararat, are isolated, or form a chain detached from the mass, like the Caucasus. We have already said that the north-east side is low and entirely open. The deep valley of Peschawer cuts its eastern side and opens a passage towards India. Not only is this plateau more accessible than that of Eastern Asia, by reason of these forms of relief, but very different from the latter, which is far from any ocean; it is bathed at its very feet, on the four corners, by inland seas, which are so many new outlets. On the south, the Arabian Sea, the Persian Gulf, and the Mediterranean; on the north, the Caspian, and the Black Seas.

Low and fertile plains, watered by twin streams, stretch at the foot of the table-land of Iran. On the south, the plains of the Euphrates and the Tigris, the unequalled fertility of which ceases with the rich alluvial lands of those rivers; on the north, the no less happy plains of Bactriana, watered by the Gihon and the Sihon. Beyond these living rivers, the steppes of the deserts establish their empire.

The climate of Western Asia no longer offers those extreme contrasts which strike us in Eastern Asia. The plateau is on the south of the central ridge, and not on the north, and enjoys a favoured climate. It is less dry, more fertile; the desert there is less continuous; these southern plains are not under the tropics; the difference between the plain and the table-land is softened.

The true Western Asia, the Asia of history, is reduced thus to a plateau flanked by two plains. Add the Soristan, which connects it with Egypt and this last-mentioned country, and you will have all the great countries of civilization of the centre of this continent; on the north the nomades of the steppes of the Caspian, on the south the nomades of Arabia and its deserts form the natural limits of the civilized world of these countries. Compared with the east, the areas are less vast, the reliefs less elevated, the nature less continental, notwithstanding its more central position, the contrasts less strongly pronounced, the whole more accessible.

Here, as we have said, is the original country of the White race, the most perfect in body and mind. If, taking tradition for our guide, we follow, step by step, the march of the primitive nations, as we ascend to their point of departure, it is at the very centre of this plateau that they irresistibly lead us. Now, it is in this central part also, in Upper Armenia and in Persia, if you remember, that we find the purest type of the historical nations. Thence we behold them descend into the arable plains, and spread towards all the quarters of the horizon. The ancient people of Assyria and B[illegible]lonia pass down the Euphrates and the Tigris into the plains of the south, and there unfold, perhaps, the most ancient of all human civilization. First, the Zend nation dwells along the Araxes, then, by the road of the plateau, proceeds to found, in the plains of the Oxus, one of the most remarkable and most mysterious of the primitive communities of Asia. A branch of the same people, or a kindred people,—the intimate connection of their language confirms it,—descends into India, and there puts forth that brilliant and flourishing civilization of the Brahmins, of which we have already spoken. Arabia and the north of Africa receive their inhabitants by Soristan. South Europe, perhaps, by the same routes, through Asia Minor; the North, finally, through the Caucasus, whence issue in succession, the Celts, the Germans, and many other tribes, who hold in reserve their native vigour for the future destinies of this continent. *There*, then, is the cradle of the White race—at least of the historical people—if it is not that of all mankind.

The civilizations of Western Asia also, as well as those of Eastern Asia, spring up in the alluvial plains, which are easily tilled, and alike connect themselves with the great rivers, and not, as in Europe, with the seas. The plains of Babylonia and of Bactriana are continental, and not maritime, like India and China. The contrasts of nature are still strongly expressed, but yet less so than in the east. There are still vast spaces, and, consequently, vast states. The religions, the political and social condition of the people, still betray the influence of a nature which man has not yet succeeded in overmastering.

The civilizations are still local, and each has its special principle;

and yet there is no more of isolation. The accessible nature of all these regions, as we have seen, makes contact easy, and facilitates their action upon each other; a blending is possible, and it takes place. The formation of great monarchies, embracing the whole of Western Asia, from India to Asia Minor, from the steppes of Turan to the deserts of Arabia, is a fact renewed at every period of their history. Assyria, Babylonia, Persia, reunite successively, under the dominion of the same conqueror, all these various nations. But no one knew so well as Alexander how to break down all the fences which kept them apart. The lofty idea which reigned in the mind of that great conqueror, that of fusing together the East and the West, carried with it the ruin of the special civilizations of the East, and the universal communication of Hellenic culture, which should combine them in one spirit, and drew the whole of that part of the world into the progressive movement which Greece herself had impressed on the countries of the West.

Egypt, alone, in her isolation, represents, up to a certain point, the nature of Eastern Asia. Yet she, too, was compelled to yield to the social and progressive spirit of Greece, which soon brought her into the circle of relations with the nations of the West.

Thus the people and the civilizations of Western Asia were saved from the isolation and egoism so fatal to China and to India. They perished in appearance, but it was only to sow among the very nations who were their conquerors, the prolific seeds of a fairer growth, of which the future should gather the fruits.

Europe, in her turn, has a character quite special, the principal features of which we have already pointed out in a former Lecture. Although constructed upon the same fundamental plan with the two Asias, it is only the peninsular headland of all this continent. Here are no more of those gigantesque forms of Eastern Asia, no more of those boundless spaces, no more of those obstacles against which the forces of man are powerless, of those contrasts which sunder the opposite natures, even to incompatibility. The areas contract and shrink; the plateaux and the mountains are lowered, and the continent opens on all sides. None of those mortal deserts to cross,—none of those impassable mountain chains, which imprison the nations. From the foot of Italy to the North-Cape, from the coasts of the Atlantic to the shores of the Caspian, there is no obstacle which a little art may not overcome without much effort. The whole continent is more accessible, it seems more wieldy, better fashioned for man.

And yet, all the contrasts of both Asias exist, but they are softened, tempered. There is a Northern world, and a Southern world, but they are less different, less hostile; their climates are more alike. Instead of the tropical plains of India, we find there the fields of Lombardy; instead of the Himalaya, the Alps; instead of the plateaux of Thibet, those of Bavaria. The contrasts are even more varied, more numerous still. The table-land of the South is broken up into peninsulas and islands; Greece and its archipelago, Italy and its isles, Spain and its sierras, are so many new individuals, exciting each other reciprocally to animation. The ground is everywhere cut and crossed by chains of mountains, moulded in a thousand fashions, in such a way as to present, within the smallest possible space, the greatest number of districts physically independent.

Add to all these advantages that of a temperate climate, rather cold than hot, requiring of men more labour and effort, and you will be satisfied that nature is nowhere better suited to exalt man, by the exertion of his powers, to the grandeur of his destination.

Nevertheless, the earliest civilized societies do not spring up in Europe; she is too far removed from the cradle of the nations, and the beginnings are less easy there. But these first difficulties once overcome, civilization grows and prospers with a vigour unknown to Asia. In Asia it is in the great plains, on the banks of the rivers, that civilization first shews itself. In Europe, it is on the peninsulas and the margin of the seas.

Europe is thus the continent most favoured, considered with respect to the education of man, and the wise discipline it exercises upon him. More than any other it calls into full play his latent forces, which cannot appear and display themselves except by their own activity. Nowhere can man better learn to subdue nature, and make her minister to his ends. No continent is more fitted, by the multiplicity of the physical regions it presents, to bring into being, and to raise up, so many different nations and peoples.

But it is not alone for the individual education of each people that Europe excels; it is still more admirably adapted than any other continent to favour the mutual relations of the countries with each other; to increase their reciprocal influence, to stimulate them to mutual intercourse. The smallness of the areas, the near neighbourhood, the midland seas thick strown with islands, *the permeability* of the entire continent—pardon me the word—everything conspires to establish between the European nations that community of life and of civilization which forms one of the most essential and precious characteristics of their social state.

America, finally, the third continent of the North, presents itself to us under an aspect entirely different. We are already acquainted with its structure, founded on a plan widely departing from that of Asia-Europe; we know that its characteristic is simplicity, unity. Add to this feature, its vast extents, its fruitful plains, its numberless rivers, the prodigious facility of communication, nowhere impeded by serious obstacles, its oceanic position, finally, and we shall see that it is made, not to give birth and growth to a new civilization, but to receive one ready made, and to furnish forth for man, whose education the Old World has completed, the most magnificent theatre, the scene most worthy of his activity. It is *here* that all the peoples of Europe may meet together with room enough to move in; may commingle their efforts and their gifts, and carry out upon a scale of grandeur hitherto unknown, the life-giving principle of modern times—the principle of free association.

The internal contrasts which assisted the development of the nations in their infancy and youth, exist not here; they would be useless. They are reduced to two general contrasts, which will preserve their importance; the coast and interior on one side, and the North and the South on the other. The last will be further softened down, when slavery, that fatal heritage of another age, which the Union still drags after it, as the convict drags his chain and ball, shall have disappeared from this free soil, freed in the name of liberty and Christian brotherhood, as it has disappeared from the fundamental principles of its law.

Thus America also seems invited, by its physical nature, and by its position, to play a part in the history of humanity, very different indeed from that of Asia and Europe, but not less glorious, not less useful to all mankind.—(*Arnold Guyot's Physical Geography*, p. 249.*

On the Aconitum ferox, Wall., *which has recently flowered in the Garden of the Edinburgh Horticultural Society.* By J. H. Balfour, M.D., F.L.S., Professor of Botany in the University of Edinburgh. (With a Plate.) Communicated by the Author.

Aconitum ferox, Wallich apud Seringe Mus. Helvet. i., p. 160, t. 15, f. 43, 44; Plant. Asiat. Rar. vol. i., p. 35, t. 41. De Candolle Prod. i., 64; Royle Flor. Himal., p. 46, 47; A. virosum, Don. Prod. Flor. Nepal., p. 196.

Nat. Ord. Ranunculaceæ, Sub.-Ord. Helleboreæ, Class Polyandria, Ord. Tri-Pentagynia.

Generic Character.—*Calyx* coloratus, pentaphyllus, foliolis æstivatione imbricatis, valde inæqualibus, postico (*galea*) maximo, concavo, cassidæformi, duobus lateralibus (*alis*) orbiculatis, duobus anticis oblongis. *Corollæ* petala quinque vel interdum pauciora, tria antica minima, unguiformia, sæpius in stamina conversa, duo postica (*cuculli*) sub galea incumbentia, longe unguiculata, basi cucullata, cucullo superne calloso, incurvo, basi in limbum oblongum emarginatum producto. *Stamina* plurima, hypogyna.

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* We trust that ere long a British edition of this remarkable volume, with illustrative sections and maps, will be added to our literature.—*Editor*.

Ovaria 3-5 libera, unilocularia. *Ovulis* ad suturam ventralem plurimis biseriatis. *Capsulæ* folliculares, membranaceæ, stylis rostratæ, intus longitudinaliter dehiscentes. *Semina* rugosa, testa crassiuscula, spongiosa, raphe valida.—Herbæ *perennes, venenatæ, in Hemisphæræ Borealis temperatis et frigidis, montanis et alpinis obviæ;* radicibus *tuberosis, tuberibus nunc fibrilliferis, nunc napiformibus;* foliis *petiolatis, palmatim tri-quinque partitis, lobis inciso-dentatis vel multifidis;* racemis *terminalibus, pedicellis e bractearum axillis solitariis, unifloris, bibracteolatis;* floribus *ochroleucis, cœruleis purpureis vel albis.* Endlicher.

Specific Character.—Floribus racemosis, paniculatis, villosis; galea semicirculari, antice acute porrecta, deorsum attenuata; cucullorum sacco longo, angusto, calcare inclinato, labio elongato, recurvo; filamentis alatis, subsagittatis, ciliatis; ovariis, capsulis, ramisque villosis; foliis quinquepartito-palmatis, subtus pubescentibus, lobis inciso-pinnatifidis, basi cuneatis, lobulis acutis divaricatis.

The plant has been found in the Himalaya at Gossain Than, at Sirmore and Kamaon, and on the summit of Sheopore in Nipal. It occupies the highest situation in the forest-belt investing the sides of the Himalaya. It flowers during the rainy season, and perfects its fruit in October and November. The name of the plant in Sanscrit is *Visha*, which means poison, and *Ativisha*, or virulent poison. In Hindustanee it is called *Vish*, *Bish*, or *Bikh*. It was introduced into the Saharunpore garden by Dr Royle, and the present specimen was raised from seeds sent by the energetic and talented superintendent, Dr William Jameson, nephew of Professor Jameson.

The specimen in the Horticultural Society's Garden (where it has flowered under Mr Evans's care), is about five feet high. *Root* perennial, having 2-3 fasciculated fusiform attenuated tubers, some of the recent ones being nearly 5 inches long and 1½ inch in circumference, dark-brown externally, white within, sending off sparse longish branching fibres. *Stem* erect, nearly round, about the thickness of a swan quill, attenuated upwards, smooth at the lower part, pubescent above where it gives off flowering branches. *Leaves* alternate, remote, deep-green above, smooth and furrowed in the course of the ribs, paler below, covered with minute vesicular-like spots, and having prominent radiating veins, which form a beautiful angular net-work; lower and middle leaves petiolate, upper ones sessile; petioles varying in length, shorter than the lamina, smooth, deeply furrowed above, especially near the laminæ, slightly swollen where they join the stem; lamina orbiculato-cordate in circumscription, palmate, deeply five-lobed, lobes incised, lobules toothed, ending in sharp points. *Bracts* trifid, the divisions being cut or entire, two empty alternate bracteoles occurring about the middle of each single-flowered pedicel. *Inflorescence* laxly panicled, the peduncles and pedicels being erect, swollen upwards and covered with a glandular pubescence. *Receptacle* of the flower swollen and oblique. *Æstivation* imbricate. *Flowers* large, blue. *Calyx* covered with glandular pubescence, helmet-shaped sepal gibbously-semicircular, prolonged in front into a short greenish point, which is turned upwards, two lateral sepals (wings) rounded, reniform with reflexed margins, lower sepals oblong, acute, deflexed, spreading, one usually larger than the other, occasionally three. *Petals* varying in size and form, upper ones cuculliform with scattered hairs and having narrowed grooved stalks ending in hollow incurved laminæ, which have their apices prolonged in a reflexed manner, other petals either wanting or mere filiform processes. *Stamens* indefinite. *Filaments* hairy, thickened below where they are margined with a broadish membrane. *Anthers* 2-lobed, with longitudinal dehiscence. *Ovaries* five, villous. *Style* single. *Stigma* obscurely 2-lobed. *Ovules* numerous, somewhat angular and winged, rugose. *Fruit* follicular, follicles oblong, villous, reticulated. *Seeds* black and pitted.

The specimen does not agree completely, more especially as regards the form of the leaves, with the figure in Wallich's *Plantæ Asiaticæ Rariores.* The variation may depend on situation, for Wallich remarks, that on Sheopore in Nipal, where he gathered the plant at the height of 10,000 feet, it was a smaller, more slender, and smoother plant than in other parts of India, with an almost simple stem, narrow segments of the leaves, and thin racemes. As it approaches higher elevations, towards the Snowy Mountains, it attains a larger size and habit, and is covered with soft greyish hairs, the divisions of the leaves become broader, the spikes larger, and the flowers more dense and numerous.

In Dr Hamilton's herbarium, in the University of Edinburgh, there are certain species of Aconite which are marked Caltha? The first, No. 1247, is called *Bisma, Bishma,* or *Bikhma,* Snowy Mountains, 1810. The second, No. 1248, *Nirbisia, Nirbishi,* or *Nirbikhi,* Snowy Mountains, 1810; pointed out by the mountaineers as a very poisonous root. There is a third in the catalogue, No. 1249, marked Oodoa, Kodoya, *Bish*, or *Bikh.* This last, according to Dr Wallich's statement, is Aconitum ferox. The specimen, however, is not to be found in Dr Hamilton's herbarium at present.

The root of the plant possesses extreme acrimony, and very marked narcotic properties. It is said to be the most poisonous of the genus, and as such has been employed in India. Wallich says, that in the Turraye, or low forest lands which skirt the approach to Nipal, and among the lower range of hills, especially at a place called Hetounra, quantities of the bruised root were thrown into wells and reservoirs, for the purpose of poisoning our men and cattle. By the vigilant precaution of our troops, however, these nefarious designs were providentially frustrated. In the northern parts of Hindustan, arrows poisoned with the root of Bikh are used for destroying tigers. The root, according to Royle, is sent down into the plains, and used in the cure of chronic rheumatism, under the name of *Meetha tellia.* Roots, apparently of this plant, were sent to Dr Christison from Madras under the name of Nabee. Pereira made a series of experiments on roots of Bikh, which had been kept for ten years in Dr Wallich's herbarium. These experiments are detailed in the Journal of Natural and Geographical Science for 1830, vol. ii., p. 235. The roots were administered to animals in the form of powder, and spirituous and watery extract. The spirituous extract was the most energetic. The poison was introduced into the stomach, the jugular vein, the cavity of the peritonæum, and the cellular tissue of the back. The effects produced were difficulty of breathing, weakness, and subsequently paralysis, which generally shewed itself first in the posterior extremities, vertigo, convulsions, dilatation of the pupil, and death apparently from asphyxia. One grain of the alcoholic extract, introduced into the peritoneal sac of a small rabbit, caused death in 9½ minutes; and a similar quantity, introduced into the cellular tissue of the left lumbar region, proved fatal in 15 minutes. Two grains and a half of the same extract, introduced into the jugular vein of a strong dog, caused death in 3 minutes.

Explanation of Plate V.

The beautiful drawing has been executed by Mr James M'Nab, Superintendent of the Royal Botanic Garden.

1. Part of a flowering panicle of Aconitum ferox. 2. Five-lobed palmate leaf. 3. Peduncle and bracts, receptacle, stamens, and the two cuculliform petals. 4. A single cuculliform petal separated. 5. Five follicles forming the fruit.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY AND HYDROLOGY.

1. *Fire-Ball at Bombay.*—On the evening of Monday the 19th February, about half-past six o'clock, just as the sun had set, and twilight was yet strong, a magnificent fire-ball was seen to shoot across the island from south-west to north-east, and burst over the mountain range beyond. It was so large, so luminous, and so rapid in its movements, that it appeared to many as if within a hundred feet or so of the ground. It was of the most beautiful greenish-white, of dazzling splendour; on bursting, the fragments were of a strong, rather darkish, red. It was seen over the whole of the island of Bombay, and at almost every intermediate part for some 300 miles into the interior. It appears to have been at a great elevation, and, as suggested by a Poonah correspondent, was probably some hundreds of miles from the nearest spectator when first seen. The volume of the mass, the length of its course, and the velocity with which it rushed along, may from this be imagined. As above observed, when first seen at Bombay it appeared as if nearly over the dockyard; in this all the observers who noticed it in different parts of the island concur. Curiously enough, we have not been favoured with a single notice of it from any one on board the ships in the harbour; from the anchorage we have no doubt it would also appear to the eastward. At Poonah, lat. 18° 30′ N., long. 72° 2′ E., it was observed at a quarter-past six at the altitude of about 30°; it was visible from Poorundhur, twenty-six miles east of Poonah. It was observed at Aurungabad, lat. 19° 45′ N., long. 75° 30′ E., as if to the south; and from Sholapore, lat. 17° 40′ N., long. 76° E., where its appearance was most carefully described as seen in a north-easterly direction. It was also carefully observed at Surat, 21° 11′ N., 73° 7′ E. It has thus been described as visible over an area of above 3° of longitude and 2° of latitude—from Bombay, 18° 53′ N., and 72° 49′ E., to Sholapore and Aurungabad; though in all likelihood it may have been observed over a much more extensive area than this, from which as yet no observations have reached us. From the explosions heard at Aurungabad it is possible that in this neighbourhood it burst. We have already alluded to the very great interest attached to notices of matters such as these, and our anxiety on all occasions to be furnished with them. With a few more notices such as those given below, we should very probably obtain the means of guessing very nearly at the distance and velocity, and course pursued by fire-balls. As we have now had abundance of time to have heard from the most remote of our outstations, and our friends have been obliging enough to respond so extensively as they have done to our call for information, we infer that the meteor was not visible much

to the southward of Sholapore or northward of Surat, or greatly to the westward of Bombay or eastward of Asseerghur—that is, betwixt the parallels of 17° and 22° and meridians of 72° and 77°, or over an area of 300 miles north and south, and as much east and west, or 90,000 square miles in all. The western margin of this space for about 30 miles is a little above the level of the sea; the eastern portion for about 250 miles varies in elevation from 1900 to 2000 feet.—(*From the Bombay Monthly Times, March* 1849.)*

2. *Great mass of Atmospheric Ice.*—A curious phenomenon occurred at the farm of Balvullich, on the estate of Ord, occupied by Mr Moffat, on the evening of Monday last. Immediately after one of the loudest peals of thunder heard there, a large and irregular-shaped mass of ice, reckoned to be nearly 20 feet in circumference, and of a proportionate thickness, fell near the farm-house. It had a beautiful crystalline appearance, being nearly all quite transparent, if we except a small portion of it which consisted of hailstones of uncommon size, fixed together. It was principally composed of small squares, diamond-shaped, of from 1 to 3 inches in size, all firmly congealed together. The weight of this large piece of ice could not be ascertained; but it is a most fortunate circumstance, that it did not fall on Mr Moffat's house, or it would have crushed it, and undoubtedly have caused the death of some of the inmates. No appearance whatever of either hail or snow was discernible in the surrounding district.—(*Ross-shire Advertiser.—Scotsman, August* 11, 1849.)

3. *Report on the Air and Water of Towns. By Dr Smith (Pro. Brit. Assoc.).*—In commencing his report, the author says, it has long been believed that air and water have the most important influence on health, and superstitions have therefore constantly attached themselves to receptacles of the one, and emanations from the other. The town has always been found to differ from the country; this general feeling is a more decisive experiment than any that can be made in a laboratory. The author proceeds to examine all the sources from which the air or water can be contaminated. The various manufactories of large towns, the necessary conditions to which the inhabitants are subjected, and the deteriorating influences of man himself are explained. If air be passed through water, a certain amount of the organic matter poured off from the lungs is to be detected in it. By continuing this experiment for three months, Dr Smith detected sulphuric acid, chlorine, and a substance resembling impure albumen. These substances are constantly being condensed upon cold bodies; and, in a warm atmosphere, the albuminous matter very soon putrifies, and emits disagreeable odours. The changes which this substance undergoes by oxidation, &c., were next examined, and shewn

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* Mr Dawson's account of the halo observed at Pictou, Nova Scotia, arrived too late; but will appear in our next, with an engraving.—*Edit.*

to give rise to carbonic acid, ammonia, sulphuretted hydrogen, and probably other gases. The ammonia generated, fortunately from the same sources as the sulphuretted hydrogen, materially modifies its influences. The consequences of the varying pressure of the atmosphere have been observed; and it is shewn, that the exhalations of sewers, &c., are poured out in abundance from every outlet where the barometric pressure is lowered. By collecting the moisture of a crowded room, by means of cold glasses, and also dew in the open air, it was found that one was thick, oily, and smelling of perspiration, capable of decomposition and production of animalcules and confervæ,—but the dew beautifully clear and limpid. Large quantities of rain-water have frequently been collected and examined by Dr Smith; and he says, I am now satisfied that dust even comes down with the purest rain, and that is simply coal-ashes. No doubt this accounts for the quantity of sulphites and chlorides in the rain, and for the soot, which are the chief ingredients. The rain is also often alkaline,—arising, probably, from the ammonia of burnt coal, which is no doubt a valuable agent for neutralizing the sulphuric acid so often formed. The rain-water of Manchester is about 2¼° of hardness,—harder, in fact, than the water from the neighbouring hills, which the town intends to use. This can arise only from the ingredients obtained in the town atmosphere; but the most curious point is the fact, that organic matter is never absent, although the rain continues for whole days. The state of the air is closely connected with that of the water; what the air contains the water may absorb,—what the water has dissolved or absorbed it may give out to the air. The enormous quantity of impure matter, filtering from all parts of a large town into its many natural and artificial outlets, does at first view present us with a terrible picture of our underground sources of water; but, when we examine the soil of a town, we do not find the state of matters to present that exaggerated character which we might suppose. The sand at the Chelsea Waterworks contains only 1·43 per cent. of organic matter, after being used for weeks. In 1827, Liebig found nitrates in twelve wells in Giessen, but none in wells two or three hundred yards from the town. Dr Smith has examined thirty wells in Manchester, and he finds nitrates in them all. Many contained a surprising quantity, and were very nauseous. The examination of various wells in the metropolis shewed the constant formation of nitric acid; and, in many wells, an enormous quantity was detected. It was discovered that all organic matter, in filtrating through the soil, was very rapidly oxidized. The presence of the nitrates in the London water prevents the formation of any vegetable matter; no vegetation can be detected, even by a microscope, after a long period. The Thames water has been examined, from near its source to the metropolis, and an increasing amount of impurity detected. In the summary to this *report,* Dr Smith states, that the pollution of air in crowded rooms is really owing to organic matter, and not merely carbonic acid,—that all the water of great towns contains organic matter,—that water purifies itself from organic matter in various ways, but particularly by converting into nitrates,—that water can never stand long with advantage unless on a large scale, and should be used when collected or as soon as filtered.

4. *On the Dilatation of Ice by Increase of Temperature.*—Three observers have undertaken to solve this problem by independent trials made in the Observatory of Poulkowa. They have found that the *linear* dilatation of the ice for 80° R. is

0·0052356	(M. Schumacher sen.).
0·0051270	(M. Pohrt).
0·0051813	(M. Moritz).

The probable error in this latter determination does not exceed ± 0·0000190. It is a result so much the more important in the science of caloric, since the only estimate hitherto known on the same subject (that of Placide Heinrich) is almost five times more considerable.

The observations made at Poulkowa shew that the dilatation of the ice is a simple linear function of temperature, and that it is equal for all possible directions in a block of ice.

The quantity of atmospheric carbonic acid increases until we reach a height of 3365·8 metres; at that elevation is the limit of a constant maximum. Farther, at greater heights, the variations in quantity of carbonic acid are less considerable than in lower places. The immediate glacier atmosphere contains less carbonic acid than the neighbourhood. The *ascending* currents of air have a greater influence in the distribution of carbonic acid than the common winds.

5. *The Buoyancy of the Water of the Dead Sea.*—About sunset, we tried whether a horse and a donkey could swim in the sea (the Dead Sea) without turning over. The result was, that, although the animals turned a little on one side, they did not lose their balance. As Mr Stephens tried his experiment earlier in the season, and nearer the north end of the sea, his horse could not have turned over from the greater density of the water there than here. His animal may have been weaker, or, at the time, more exhausted than ours. A muscular man floated nearly breast-high, without the least exertion.

A horse taken into the bay could, with difficulty, keep himself upright. Two fresh hen-eggs floated up one-third of their length. They would have sunk in the water of the Mediterranean or the Atlantic.

The water of the sea was very buoyant; with great difficulty I kept my feet down; and when I laid upon my back, and, drawing up my knees, placed my hands upon them, I rolled immediately over.

Tried the relative density of the water of this sea and of the Atlantic,—the latter from 25° N. latitude, and 52° W. longitude; distilled water being as 1. The water of the Atlantic was 1·02, and of this sea 1·13. The last dissolved $\frac{1}{11}$, the water of the Atlantic $\frac{1}{6}$, and the distilled water $\frac{5}{11}$ of its weight of salt. The salt used was a little damp. On leaving the Jordan, we carefully noted the draught of the boats. With the same loads, they drew 1 inch less water when afloat on this sea than in the river.*—(*Expedition to the Dead Sea and the Jordan. By W. F. Lynch.*)

6. *Currents in the Gut of Gibraltar.*—Some curious investigations have been for some time carried on in the Gut of Gibraltar, by M. Coupvent des Bois. He has proved, as a certainty, the existence of a superficial current flowing from the ocean into the Mediterranean, and of a deep under current flowing from the Mediterranean into the ocean. He has also ascertained that between these two currents there exists a bed of water which is in perfect repose.—(*Athenæum*, No. 1138, p. 842.)

GEOLOGY.

7. *Barrande on the Trilobites of Bohemia.*—Sir Roderick Murchison has recently received a letter from M. Barrande of Prague, who is preparing a work on the Silurian System of Bohemia, and who in studying the numerous trilobites which he has collected in that country, has made a remarkable discovery in respect to these the most ancient fossil crustaceans in the crust of the globe. M. Barrande has traced for the first time the development of a trilobite (his *Sao hirsuta*) from its embryonary state to its adult condition; and has observed twenty successive stages, during which this one species undergoes very remarkable changes of organization, passing from a simple disc-like body to a fully formed trilobite with seventeen free thoracic segments and two caudal joints. This discovery is not only most interesting to physiologists, but highly important to geologists, as diminishing the number of the so-called species; it being ascertained that in a work recently published by MM. Hawle and Corda upon the trilobites of Bohemia, the authors made no less than *ten genera* and *eighteen species* out of a part only by the stages of metamorphosis of the *Sao hirsuta* (Barr.)—(*Athenæum*, No. 1132, p. 696, *7th July* 1849.)

8. *The Fossil Foot-marks of the United States, and the Animals that made them. By Edward Hitchcock, D.D., LL.D., President of Amherst College, and Professor of Natural Theology and Geology. (From the Transactions of the American Academy*

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* Since our return, some of the water of the Dead Sea has been subjected to a powerful microscope, and no animalculæ or vestige of animal matter could be detected.

of Arts and Sciences, 2d Ser., vol. iii. Boston, 1848.)—This elaborate memoir extends to 128 pages quarto, and is illustrated by 24 plates, together with a large table, giving a general view of the distinctive characters of the species. The learned author has pursued the course usual in palæontology, of distinguishing the genera and species of the *animals* indicated by the fossil remains, and naming them accordingly. Although the remains are but foot-marks, they point out, under the guidance of the unerring principles of comparative anatomy, the habits of several animals, the classes to which they pertain, and the peculiarities, to some extent, of the species. These characters have been seized, and upon them the descriptions and names are based. 51 species are included in the memoir, 12 of which are of quadrupeds, 4 probably of lizards, 2 chelonian, 6 batrachian, 2 annelids or molluscs, 34 bipeds, 3 doubtful; and of the bipeds 8 were thick-toed tridactylous birds, 16 were narrow-toed tridactylous or tetradactylous birds, 2 were batrachian, and the remaining 8 either birds or reptiles, and probably the latter. We have to defer to our next number a farther account of the genera and species.—(*American Journal of Science and Arts*, vol. viii., No. 22, 2d Series, *July* 1849, p. 151.)

9. *Fossil Foot-marks of a Reptilian Quadruped below Coal.*—At a late hour we have received the following letter from Mr Isaac Lea (dated Philadelphia, June 17), on Foot-prints in Pennsylvania in rock below the coal; a further notice is necessarily deferred to our next number:—"I am sure it will greatly interest you to learn that it has been my good fortune to have discovered 'fossil foot-marks' of a *reptilian quadruped* in the series below any heretofore observed. In a late visit to the southern coal-field of Pennsylvania, while making some geological investigations, I found six distinct double impressions in regular progression, in the *old red sandstone.* These were accompanied by numerous 'ripple-marks,' and 'pits of rain-drops,' over the whole exposed surface of the rock. The lowest heretofore observed, I believe, are of the *Cheirotherium*, described by Dr King in the coal-formation near Greensburg Pa., and those mentioned by Dr Logan in the same formation of Nova Scotia. The name I have proposed for this reptile is *Sauropus primævus.*"—(*American Journal of Science and Arts*, vol. viii., 2d Series, No. 22, p. 160.)

MINERALOGY.

10. *Emery Formation of Asia Minor. By J. Lawrence Smith.*—The following communication, received in a letter from Dr Smith, dated Constantinople, January 5, 1849, is a translation of a communication addressed by him to Elie de Beaumont.

"These lines are written with reference to an extract of a letter from M. Pierre de Tchihatcheff, published in the Comptes Rendus de l'Académie des Sciences, the 20th March 1848. It is only lately that my attention was attracted by this letter, more especially by the last phrase ('I have communicated all the necessary indications to Mr Lawrence Smith, American mineralogist in the service of the *Porte*, and he proposes to *develop my discovery*'); because, in reading this it would appear that I was entirely ignorant of the existence of the emery alluded to, in Asia Minor, and that the first information given me was by M. Tchihatcheff, on returning from his voyage in December 1847. I wish in no way to affect his right to the observations upon the emery which he found between *Eski-hissar* and *Melas*, in December 1847, situated more southerly than any of the localities to which my own observations had extended; but I am equally desirous to have my own discoveries attributed to me.

"It was in the month of November 1846 that a merchant in Smyrna shewed me specimens of emery, said to come from Kula (about 80 miles east of Smyrna). The importance of this mineral led me, early in 1847, to visit Smyrna for the purpose of investigating this matter. On my second visit to this city I was shewn other specimens coming from the neighbourhood of Ephesus, and which, being nearer than *Kula*, I at once visited. About 12 miles to the east of Ephesus, near to the village of *Gumuchkeny*, and on the summit of *Gumuchdagh*, I discovered emery *in situ;* however, before arriving there, I saw this same mineral more or less scattered over the country. The Turkish Government, to whom I communicated the importance of this discovery, sent a commission with me to examine the region in the *month of May* 1847, a fact which was announced publicly in the *Journal of Constantinople*, the 16th May 1847, in the following words:—'It is some time since Monsieur le Docteur Smith, American mineralogist, of whom we have had frequent occasion to speak, discovered at Magnesia, near to Gumuchkeny, an *emery mine*, and of which he brought specimens to Constantinople. The Government has sent to the place a commission composed of Dr Smith and some of the officers of the Imperial Powder Works, to examine thoroughly into the importance of this mine, and, according to the report that will be made, the Government will decide on the steps to be taken with reference to it,' &c.

"On our return, and after we had made our report on the development of the emery formation in three distinct places, distant from each other, to wit, in Gumuchdagh; near to Kula; and to the north of Smyrna; the monopoly of this mineral was offered for sale, and purchased first by Mr Langdon of Smyrna, and subsequently by the house of Abbott and Company, for the sum of 12,000,000 piastres per annum (55,000 dollars), and about the month of August some 800 tons from the summit of Gumuchdagh were in England. Thus when M. Tchihatcheff thought to have discovered this emery for the *first time in situ*, in December 1847, and when he wrote to you the following words:—'The brilliant prospect that Asia Minor presents in reference to this mineral, and which I am happy to have *first pointed* out, &c.,' and at the end,—'I have communicated the necessary indications to Mr Lawrence Smith, American mineralogist in the service of the Porte, who proposes to develop my discovery;'—I say, when this was written to you, already nearly a year had expired since I had made the discovery ; it was publicly known by the announcement in the Journal of Constantinople, the 16th May 1847, and the "brilliant prospect" was already appreciated by the Turkish Government, and 800 tons from the summit of Gumuchdagh were in England. Wherefore I claim the priority of the discovery of emery in Asia Minor *in situ*, and to have been the first to have made known publicly this discovery to the scientific world.

The reason why I did not make this discovery known to the scientific world was because I intended to make a complete memoir on this subject after an examination of certain points which is not yet finished ; I will mention only one or two to which my investigations have led me ; they are the discovery of the existence of the oxide of zirconium in emery, and of a new mineral that I have found associated with emery coming from all the localities of Asia Minor and of Naxos. It is a micaceous mineral having for composition silex 30, alumina 50, zirconia 4, lime 13, oxide of iron, manganese, and potash 3. I have decided to call it *Emerylite*, and to give at some future time a full description of it.

"One word upon the aspect of emery, which M. Tchihatcheff has compared to the hydrated oxide of iron. In all my observations I have not yet seen a specimen that can be compared to this oxide, even at first sight it resembles more nearly the protoxide, the silicates, and anhydrous oxides of iron. The fracture is irregular, except in a species of inferior quality from Gumuchdagh, which has a conchoidal fracture, and the aspect of black limestone. I have made other observations more or less interesting, but I reserve them for another time."—(*American Journal of Science and Arts*, 2d Series, vol. vii., No. 20, p. 283.)*

11. *Chrome and Meerschaum of Asia Minor ; By J. Lawrence Smith.* (Communicated with the preceding.)—In my journey to the south of Broosa (Anatoly, Asia Minor), I crossed a formation of serpentine and other magnesian rocks of considerable extent. Fifty miles from this I discovered *chromate of iron* disseminated in these rocks; and ten or fifteen miles further south (near the city of Harmanjick), there is an abundant deposit of this mineral. A circumstance worthy of remark, is, that this chromate of iron (the first that has been discovered in Asia Minor) is found in serpentine as elsewhere. This important fact can explain, to a certain extent, the formation of this chromate. It is well known that serpentine contains all the elements of chromate of iron, which, during the consolidation of this rock, might separate themselves by the force of segregation, so well known to operate in

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* It is to be regretted that Mr Smith does not give an intelligible description of his emery.—*Editor Ed. Phil. Jour.*

many geological phenomena. Two facts, which seem to confirm this supposition, are, first, the existence of the chromate of iron in masses, and not in veins; and, secondly, the pale colour of the serpentine associated with the chromate. One small specimen that I have, consists of a white rock, composed principally of carbonate of magnesia, in which small specks of chromate of iron are visible. It is possible that this carbonate is the result of the decomposition of the serpentine at the surface, by the action of water containing carbonic acid. It is only at this locality that I found crystals of the chromate, octahedral, but very small.

This discovery is of great importance to the arts, and to the Turkish government, which proposes exploring the mine.

In quitting the locality of chrome, and going north-east, I traversed, in several places, the serpentine containing veins of carbonate of magnesia, quite pure; and this occurs until we arrive at the plains of *Eskihi-sher*. It is from different parts of this plain that comes the meerschaum most esteemed in the arts. Its geological position is very different from what I had expected. The plain in which it is found is a deposit of drift; a valley filled up with the *debris* of the neighbouring mountains, consolidated by lime in which I found no fossils.

The meerschaum is found in this drift in masses more or less rounded; the other pebbles are fragments of magnesian and hornblende rocks.

I have examined, with care, the neighbouring mountains which surround the plain, and have found that the rocks are of the same nature as the pebbles in the plain, except those of the meerschaum; but, on the other hand, I found carbonate of magnesia in the mountains, which is not to be found in the plains. And this makes me suppose that the meerschaum owes its origin to the carbonate of magnesia of the mountains, decomposed after its separation, by water containing silica.

If this supposition be true, we should naturally find meerschaum which, not being completely altered, contains the carbonate of magnesia. A chemical examination of several specimens has served to establish this fact. I have the honour to send you a specimen taken at the depth of ten feet; and if you desire to make the experiment yourself, put a small piece of the specimen, well cleaned, in hydrochloric acid. You will have immediately an effervescence which will continue for some time; the piece will not change its form, it only absorbs the acid; the solution will be found to contain chloride of magnesium nearly pure. Another proof that the meerschaum probably owes its origin to the carbonate of magnesia, is, that I have found attached to the meerschaum, serpentine, similar to that found in contact with the carbonate of magnesia of the mountains.

The meerschaum of *Eskihi-sher* differs completely from several other specimens that I have seen coming from the localities, and which exist in the fissures of rocks. It is certain that the quality of the first is most esteemed.—(*American Journal of Science and Arts*, vol. vii., Second Series, No. 20, March 1849, p. 285.)

12. *Randanite, a native Hydrated Silica from Algiers. By M. Salvetat.*—This hydrated silica exists abundantly near Algiers, and was taken for Kaolin. It is pulverulent and friable, forming an excessively light powder. It is infusible, but loses colour and becomes grayish, contracting a little. It gives out water at 16° C., but still retains a portion at 100° C., losing the whole only at an intense heat. It was found to consist of 80 parts of gelatinous silica, 9 of water, 6·48 of insoluble silica, with 1·41 alumina, 0·55 oxide of iron, 0·56 lime, 2·00 of potash, soda, and loss, and *a trace* of magnesia. Of the water 4·04 per cent. escaped at 16° C., and 3·96 at 100° C., and 1 per cent. is combined with the alumina.

The composition resembles that of a similar material from Ceyssat, and near Randan, in the Puy de Dome, analysed by M. Fournet. This chemist obtained in his analysis, gelatinous silica 87·20, water, carbonic acid, and organic matters 10·00, alumina and oxide of iron 2·00, sand by decantation 0·80, with traces of lime, magnesia, &c.—(*Ann. de Ch. et de Phys.*, November 1848, t. xxiv., p. 348).

13. *Analysis of Lardite from near Voigtsberg, in Saxony.*—Lardite, which has been referred to agalmatolite, is an anhydrous magnesian silicate, consisting, according to Karsten's analysis, of silica 66·02, magnesia 31·94, protoxide of iron 0·81, soda and potassa 0·75, loss by ignition 0·20, chloride of sodium and sulphate of potash, a *trace*=99·72. It whitens before the blow-pipe, and in a tube gives no trace of moisture, but exhales a disagreeable odour, like many other magnesian minerals. In the exterior flame it becomes wax yellow. It dissolves slowly but completely in borax, forming a glass which is pale yellow when hot, but becomes white on cooling. Density 2·795.—*Karsten* (*Jour. für Prak. Chem.*, xxxvii., s. 162.)

14. *Neolite, a new Mineral.*—Neolite is a talc-like mineral from some old mines near Arendal, Norway, where it occurs as modern incrustations in fissures, and on detached stones. It is often crystalline, either *in folia* or in concentric fibrous aggregations like Wavellite. It is greenish, with a greasy lustre, and a specific gravity 2·77 after long desiccation. Hardness, that of talc. The analyses vary much. In one, Scheerer obtained silica 52·28, alumina 7·33, magnesia 31·24, protoxide of iron 3·79, protoxide of manganese 0·89, lime 0·28, water 4·04=99·85. In another, silica 47·35, alumina 10·27, magnesia 24·73, protoxide of iron 7·92, protoxide of manganese 2·64, water 6·28 = 99·19.—*M. Scheerer.*

15. *On Völknerite, a new Mineral from the Mines of Schischimsk. By M. Hermann* (*Jour. f. Prak. Ch.*, xl. 11).—Völknerite occurs in white pearly laminæ on talc-slate, and sometimes in hexagonal tables, with a perfect basal cleavage. Feel greasy; density 2·04; composition $\text{Al}\ 3\ \text{H} + 6\ (\text{Mg}\ 2\ \text{H})$.

16. *Analysis of Pyrophyllite of Spaa. By M. Rammelsberg* (*Pogg. Annalen*, lxviii., 505).—The analysis afforded, silica 66·14 alumina 25·87, magnesia 1·49, lime 0·39, water 5·59 = 99·48.

17. *Analysis of Talc of Rhode Island and Steatite of Hungary. By M. A. Delesse* (*Rev. Sci. et Indust.*, xxv., 107).—The talc of Rhode Island occurs in large clear foliated masses. It has two optical axes intersecting at a small angle; density = 2·5657; after calcination = 1·64. Hardness = 1; after calcination = 6, so that it scratches glass, although with some difficulty. It exfoliates when heated. On analysis it afforded silica 61·75, magnesia 31·68, protoxide of iron 1·70, water 4·83 = 99·96.

18. *On a new Hydrosilicate of Alumina. By MM. Damour and Salvetat* (*Ann. de Ch. et de Ph.*, 3e *Ser.*, xxi., 376).—This mineral occurs massive in nests in a brownish clay near Montmorillon (Vienna). It has a soapy feel, and a pure rose colour, and becomes plastic in water. *Composition*, according to Damour, silica 50·04, alumina 20·16, sesquioxide of iron 0·68, lime 1·46, potash 1·27, magnesia 0·23, water 26·00. It is hence allied to Halloysite.

19. *Philippsite and Gismondine. By M. Marignac* (*Ann. de Ch. et de Phys.*, 3e *ser.*, xiv., 41).—Marignac separates these species, which Kobell and Brooke had united. Under *Gismondine* he includes specimens having an octahedral form, and rarely mammillated, and faces not striated; and under Philippsite, those whose crystals have a rectangular prismatic form terminated by a four-sided pyramid, with the faces striated in two directions oblique to one another. Density of Gismondine 2·265, of Philippsite 2·213.

20. *On the Composition of Heulandite. By M. Damour* (*Comptes Rendus*, xxii., 926; *Annuaire de Chim.*, 1847).—Damour has detected in Heulandite a portion of soda and potash which simplifies the formula. His analysis gives, silica 59·64, alumina 16·33, lime 7·44, soda 1·16, potash 0·74, water 14·33 = 99·64. Hence, this mineral differs from stilbite only in the proportion of water.

21. *On the Identity of Osmelite and Pectolite* (*Annuaire de Chem.*, 1848, p. 166).—An analysis by M. Adam indicates that osmelite of Breithaupt is identical with Kobell's pectolite. It contains, silica 52·91, lime 32·96, protoxide of manganese 1·44, soda 6·10, potash 2·79, alumina and oxide of iron 0·54, water 4·01.

22. *On Disterrite, from the Valley of Fassa in Tyrol. By M. Von Kobell* (*Jour. of Prak. Ch.*, xli, 154; *Annuaire de Ch.*, 1848, 173).—Disterrite crystallizes in hexagonal prisms, cleaving parallel to the base, and has a pearly lustre on the terminal faces, with a vitreous lustre on the sides of the prism. H = 5 to 6½ (Breithaupt's scale); sp. gr. 3·042 – 3·051; *Composition*, silica 20·00, alumina 43·22, peroxide of iron 3·60, magnesia 25·01, lime 4·00, potash 0·57, water 3·60.

23. *On Glaucophane. By* M. Hausmann (*Jour. of Prak. Ch.*,

xxxiv., 238; *Annuaire de Chimie*, 1846, p. 271).—Glaucophane comes from the island of the Cyclades, and resembles indicolite. It has a prismatic foliated structure, a pure blue colour seen by refraction; sp. gr. 3·103 – 3·113; powder feebly attracted by the needle. The mean result of two analyses is as follows:—silica 56·49, alumina 12·23, protoxide of iron 10·91, protoxide of manganese 0·50, magnesia 7·97, lime 2·25, soda with traces of potash 9·28 = 99·63. It resembles Wichtyne from Finland in composition.

BOTANY.

24. *Chinese Method of Colouring Green Teas.*—During a visit which I paid to a tea manufactory in the city of Shanghae, I happened to meet some merchants who came from the celebrated green tea district of Wheychou. Thinking this a good opportunity for obtaining some information regarding the mode of colouring green teas, and, as I was accompanied by Mr M. Donald, an excellent Chinese scholar, I had some questions put to them on this subject. They would not acknowledge that any colouring matter was used in the manufacture of their teas, and pretended to laugh at the idea of such a thing. They said, moreover, that they were aware the practice of colouring was a common one about Canton, where inferior teas were made, but that they never coloured their teas in Wheychou. They then skilfully enough tried to change the subject by telling us, that we should not give credence to all we heard. "If *we* did so," said they, "we would make some strange mistakes with regard to the productions and manufactures of your country. For example," they continued, "it is commonly reported that you buy your teas in order to convert them into opium, and resell them in that form to us. Now, we do not believe that you do that; and neither should you believe all you hear about the colouring of our green teas." After giving us this sage advice, they asked us very gravely, how we used this tea in England,—and if it was true that we had the leaves boiled and beat up with sugar and milk!

It is, however, a difficult thing to get the truth out of a Chinaman: and from information which I had received, I knew quite well that our Wheychou friends were deceiving us in the present instance. Shortly afterwards I had an opportunity of seeing the whole process; and as it is one of considerable interest, I noted it down at the time with great care, and now send you a copy of my observations.

The superintendent of the tea makers managed the colouring part of the business himself. In the first place, he procured a portion of indigo, which he threw into a porcelain bowl, not unlike a chemist's mortar, and crushed it into a fine powder. He then burned a quantity of gypsum in the charcoal fires which were roasting the tea. The object of this was to soften the gypsum, in order that it might easily be pounded into a fine powder in the same manner as the indigo had been. When taken from the fire it readily crumbled down, and was reduced to powder in the mortar. These two substances having been thus prepared, were then mixed up in the proportion of four parts gypsum to three of indigo, and together formed a light-blue powder, which, in this state, was ready for use. This colouring matter was applied to the tea during the last process of roasting. The Chinese manufacturer having no watch to guide him, uses a joss stick* to regulate his movements with regard to time. He knows exactly how long the joss stick burns, and it, of course, answers the purpose of a watch. About five minutes before the tea was taken out of the pans, the superintendent took a small porcelain spoon, lifted out a portion of the colouring matter from the bason, and scattered it over the tea in the first pan; he did the same to the whole, and the workmen turned the leaves rapidly round with their hands, in order that the colour might be well diffused.

During this part of the operation, the hands of the men at the pans were quite blue. I could not help thinking, that if any drinker of *green tea* had been present during this part of the process, his taste would have been corrected; and, I hope, I may be allowed to add, improved. It seemed perfectly ridiculous, that a civilized people should prefer these dyed teas to those of a natural green. No wonder that the Chinese consider the nations of the West as "barbarians." One day Mr Shaw, a merchant in Shanghae, asked the Wheychou Chinamen their reasons for dyeing their teas; they quietly replied, that as foreigners always paid a higher price for such teas, they, of course, preferred them; and that such being the case, the Chinese manufacturer could have no objection to supply them.

I took some trouble to ascertain precisely the quantity of colouring matter used in the process of dyeing green teas; certainly not with the view of assisting others, either at home or abroad, in the art of colouring, but simply to shew green tea drinkers in England, and more particularly in the United States of America, what quantity of gypsum and indigo they eat or drink in the course of a year. To 14½ lb. of tea were applied rather more than an ounce of colouring matter. For every hundred pounds of green tea which are consumed in England or America, the consumer really eats more than half a pound of gypsum and indigo; and I have little doubt, that in many instances Prussian blue is substituted for indigo. And yet, tell these green tea drinkers, that the Chinese eat dogs, cats, and rats, and they will hold up their hands in amazement, and pity the taste of the poor Celestials.

In five minutes from the time of the colour being thrown into the pan, the desired effect was produced. Before the tea was removed, the superintendent took a tray and placed a handful from each pan upon it. These he examined at the window, to see if they were uniform in colour; and if the examination was satisfactory, he gave the order to remove the tea from the pans, and the process was complete. It sometimes happened, that there was a slight difference amongst the samples; and in that case, it was necessary to add more colour, and, consequently, keep the tea a little longer in the pan.—(*R. F.*, *Athenæum*, No. 1136, p. 790.)

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* An incense burner.

ZOOLOGY.

25. *Additional Observations on a new living Species of Hippopotamus of Western Africa. By S. G. Morton, M.D., Penn. and Edin., Vice-President Acad. Nat. Sci. Philadelphia.* (*From the Journal of the Acad. Nat. Sci. Philadelphia*, 1849.)—This new species of Hippopotamus was first described by Dr Morton in the Proceedings of the Academy, for February 1844, and there named *H. minor.*[*] As this name was previously used by Cuvier for a fossil species, it is now changed to Hippopotamus (Tetraprotodon) *Liberiensis*. The animal is slow and heavy in its motions, and weighs 400 to 700 pounds. It lives on the river St Paul's, a stream that rises in the mountains of Guinea, and passing through the Dey country and Liberia, empties into the Atlantic to the north of Cape Messurado. The description of the animal by Dr Morton is drawn from two skulls in his possession, the only specimens which have hitherto been brought from the African coast.—(*The American Journal of Science and Arts*, vol. viii., No. 22, p. 152.)

ARTS.

26. *The Portland Vase.*—An account of the Portland Vase was published by the late Mr Wedgwood, the father of the potteries, and an accomplished philosopher; it is, like its author, truthful and accurate. On this famed vase being offered for sale, Wedgwood, considering that many persons, to whom the original was unattainable, might be willing to pay a handsome price for a good imitation of it, endeavoured to purchase it, and for some time continued to offer an advance upon each bidding of the Duchess of Portland, until, at length, his motive being ascertained, he was offered the loan of the vase on condition of withdrawing his opposition. Consequently, the Duchess became the purchaser at the price of eighteen hundred guineas. It is stated that a limited number of copies were sold at fifty guineas each, and that the model cost five hundred guineas; probably, the celebrated

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[*] See Silliman's Journal, xlvii., p. 406, where woodcuts are given.

Flaxman was the artist who was so liberally rewarded. Sir Joseph Banks and Sir Joshua Reynolds bore testimony to the excellent execution of these copies, which were chased by a steel rifle, after the bas-relief had been wholly or partially fired.—(*Curiosities of Glass-making, by Apsley Pellatt*, p. 21.)*

MISCELLANEOUS.

27. *On the Tricks of Fire-eaters and Conjurors.*—M. P. H. Boutigny, whose beautiful experiments on the spheroidal condition of water created so much interest at the meeting of the British Association at Cambridge, has lately been pressing his researches on heat in a somewhat novel direction. He has now proved that metals in a melted state have, in a remarkable manner, the repulsive force of incandescent surfaces, and that the tricks of fire-eaters and conjurors belong to a high class of physical facts. He says, "I have made the following experiments:—I divided or cut with my hand a jet of melted metal of five centimetres, which escaped by the tap. I immediately plunged the other hand into a pot filled with incandescent metal which was truly fearful to look at. I involuntarily shuddered, but both hands came out of the ordeal victorious. * * * * I shall of course be asked," he continues, "What are the precautions necessary to prevent the disorganizing action of the incandescent mass? I answer none. Have no fear—make the experiment with confidence—pass the hand rapidly, but not too rapidly, in the metal in full fusion. The experiment succeeds perfectly when the skin is moist, and the dread usually felt at facing masses of fire supplies the necessary moisture; but by taking some precaution, we may become truly invulnerable. The following succeeds best with me: I rub my hands with soap, so as to give them a polished surface; then, at the instant of trying the experiment, I dip my hand into a cold solution of sub-ammoniac saturated with sulphurous acid." The experiment has been tried by Boutigny with melted lead, bronze, and cast-iron.—(*Athenæum*, No. 1138, p. 842.)

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* One of these beautiful copies is preserved in the Natural History Museum of the University of Edinburgh.

List of Patents granted for Scotland from 22*d June to* 22*d September* 1849.

1. To David Smith, of the city of New York, in the United States of America, lead-manufacturer, and a citizen of the said United States, "certain new and useful improvements in the means of manufacturing certain articles in lead."—24th June 1849.

2. To Walter Neilson, of Hyde Park Street, in the city of Glasgow, North Britain, engineer, "an improvement or improvements in the application of steam for raising, lowering, moving, or transporting heavy bodies."—24th June 1849.

3. To Edmund Grundy, of Bury, in the county of Lancaster, woollen manufacturer, and Jacob Farrow, of the same place, manager, "certain improvements in machinery or apparatus for preparing wool for spinning, and also improvements in machinery or apparatus for spinning wool and other fibrous substances."—25th June 1849.

4. To Robert William Lawrie, of Carlton Place, in the city of Glasgow, North Britain, "improvements in means or apparatus to be employed for the preservation of life and property, such improvements, or parts thereof, being applicable to various other articles of furniture dress, and travelling apparatus."—29th June 1849.

5. To Edward Hawkins Payne, of Great Queen Street, in the county of Middlesex, coach-lace manufacturer, and Henry William Currie, engineer, "improvements in the manufacture of coach-lace, and other similar looped or cut pile fabrics."—9th July 1849.

6. To Robert Urwin, of Ashford, in the county of Kent, engineer, "certain improvements in steam-engines, which may in whole or in part be applicable to pumps and other machines not worked by steam-power." —9th July 1849.

7. To William Wilson junior, residing at Campbellfield of Glasgow, in the county of Lanark, Scotland, "improvements in casting plastic tubes or tiles."—10th July 1849.

8. To James Godfrey Wilson, of Millman Row, Chelsea, in the county of Middlesex, engineer, "certain improvements in obtaining perfect combustion, and in apparatus relating thereto, the same being applicable generally to furnaces and fire-places, as also to other purposes where inflammable matter or material is made use of."—11th July 1849.

9. To George Benjamin Thorneycroft, of Wolverhampton, in the county of Stafford, iron-master, "improvements in manufacturing railway tyres, axles, and other iron where great strength and durability is required."—16th July 1849.

10. To William Kenworthy, of Blackburn, in the county of Lancaster, cotton-spinner, "certain improvements in power-looms for weaving."—16th July 1840.

11. To William Crofton Moat, of Upper Berkeley Street, in the county of Middlesex, surgeon, "improvements in engines to be worked by steam, air, or gas."—16th July 1849.

12. To Edward Ives Fuller, of Margaret Street, Cavendish Square, in the county of Middlesex, carriage-builder, and George Tabernacle, of Mount Row, Westminster Road, in the county of Surrey, coach-iron-founder, "certain improvements in metallic springs for carriages."—17th July 1849.

13. To John Grantham, of Liverpool, engineer, "improvements in sheathing ships and vessels."—18th July 1849.

14. To Peter Augustine Godefrey, of Wilson Street, Finsbury Square, chemical colour-manufacturer, "certain improvements in dressing and finishing woven fabrics."—18th July 1849.

15. To Joseph Eccles, of Moorgate Fold-Mill, near Blackburn, in the county of Lancaster, cotton-spinner and manufacturer, and James Bradshaw and William Bradshaw, of Blackburn, in the same county, watch-makers, "certain improvements in and applicable to looms for weaving various descriptions of plain and ornamental textile fabrics."—19th July 1849.

16. To James White, of Lambeth, in the county of Surrey, civil engineer, "improvements in machinery or apparatus for sowing seed."—25th July 1849.

17. To James Green Gibson, of Ardwick, near Manchester, in the county of Lancaster, machinist, "certain improvements in machines used for preparing to be spun and spinning cotton and other fibrous substances, and for preparing to be woven and weaving such substances when spun."—30th July 1849.

18. To Andrew Peddie How, of the United States, but now residing at Basinghall Street, in the city of London, engineer in the United States Navy, "an instrument or instruments for ascertaining the saltness of water in boilers," being a communication from abroad.—1st August 1849.

19. To Hugh Lee Pattinson, of Washington House, Gateshead, in the county of Durham, chemical manufacturer, "improvements in manufacturing a certain compound or certain compounds of lead, and the application of a certain compound or certain compounds of lead to various useful purposes."—6th August 1849.

20. To François Amedee Remond, of Birmingham, "improvements in machinery for folding envelopes, and in the manufacture of envelopes."—6th August 1849.

21. To Richard Kemsley Day, of Stratford, in the county of Essex, hydrofuse manufacturer, "improvements in the manufacture of emery paper, emery cloth, and other scouring fabrics."—7th August 1849.

22. To John Thom, of Ardwick, near Manchester, in the county of Lancaster, calico-printer," improvements in cleansing, scouring, or bleaching silk, woollen, cotton, and other woven fabrics and yarns, and in aging fabrics and yarns when printed."—7th August 1849.

23. To Joseph Findlay, of New Snedden Street, Paisley, in the county of Renfrew, North Britain, manufacturer, and Andrew Wilkie, of the same place, turner, "an improvement or improvements in machinery or apparatus for turning, cutting, shaping, and reducing wood, or other substances."—10th August 1849.

24. To Edward Lord, of Todmorton, in the county of Lancaster, machinist, "certain improvements in machinery or apparatus applicable to the preparation of cotton and other fibrous substances."—15th August 1849.

25. To James Thomson Wilson, of Middlesex, chemist, "improvement in the manufacture of sulphuric acid and alum."—15th August 1849.

26. To Pierre Armand Le Comte de Fontainemoreau, of No. 4 South Street, Finsbury (English and Foreign Patent Office), "certain improvèments in weaving," being a communication from abroad.—22d August 1849.

27. To James Naysmith, of Patricroft, near Manchester, in the county of Lancaster, engineer, "certain improvements in the methods of, and apparatus for communicating and regulating the power for, driving or working machines employed in manufacturing, dyeing, printing, and finishing textile fabrics."—24th August 1849.

28. To Job Cutler, of Birmingham, in the county of Warwick, gentleman, "certain improvements in the manufacture of metallic tubes or pipes."—28th August 1849.

29. To Henry Gilbert, of Suffolk Place, Pall-Mall East, in the county of Middlesex, surgeon, "an improved mode, or improved modes, of operating in dental surgery, and improved apparatus or instruments to be used therein."—28th August 1849.

30. To William Chamber Day, of Birmingham, in the county of Warwick, iron-founder and weighing-machine manufacturer, "improvements in machinery for weighing."—29th August 1849.

31. To James Robertson, of Huddersfield, in the county of York, orchil and cudbear manufacturer, "improvements in preparing or manufacturing orchil and cudbear."—29th August 1849.

32. To Robert William Thomson, of Leicester Square, in the county of Middlesex, civil engineer, "certain improvements in writing and drawing instruments."—31st August 1849.

33. To John Holland, of Larkhall Rise, in the parish of Clapham, in the county of Surrey, gentleman, "a new mode of making steel," being a communication from a foreigner residing abroad.—11th September 1849.

34. To Edwin Heywood, of Glasburn, in the county of York, designer to Messrs Thomas and Mathew Bairstow, of Sutton, in the county aforesaid, "improvements in plain and ornamental weaving."—11th September 1849.

35. To Robert Plummer, of the town and county of Newcastle-upon-Tyne, manufacturer, "certain improvements in machinery, instruments, and processes, employed in the preparation and manufacture of flax and other fibrous materials."—12th September 1849.

36. To William Boggett, of Saint Martin's Lane, in the county of Middlesex, gentleman, "improvements in heating and evaporating fluids, and in obtaining and applying motive power."—14th September 1849.

37. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "certain improvements in steam-boilers," being a communication from a foreigner residing abroad.—17th September 1849.

38. To James Goodier, of Mode Wheel, Manchester, in the county of Lancaster, miller, "certain improvements in mills for grinding wheat and other grain."—17th September 1849.

39. To Alexander Haig, of 52 Smith Street, Stepney, in the county of Middlesex, engineer, "improved apparatus for exhausting and driving atmospheric air and other gases, and for giving motion to other machinery."—18th September 1849.

40. To Sir John M'Neill, Knight, of Dublin, and Thomas Barry, of Lyons, near Dublin, mechanic, "improvements in locomotive engines, and in the construction of railways."—19th September 1849.

41. To William Henry Phillips, of York Terrace, Camberwell New Road, in the county of Surrey, engineer, "improvements in extinguishing fire, in the preparation of materials to be used for that purpose, and improvements to assist in saving life and property."—19th September 1849.

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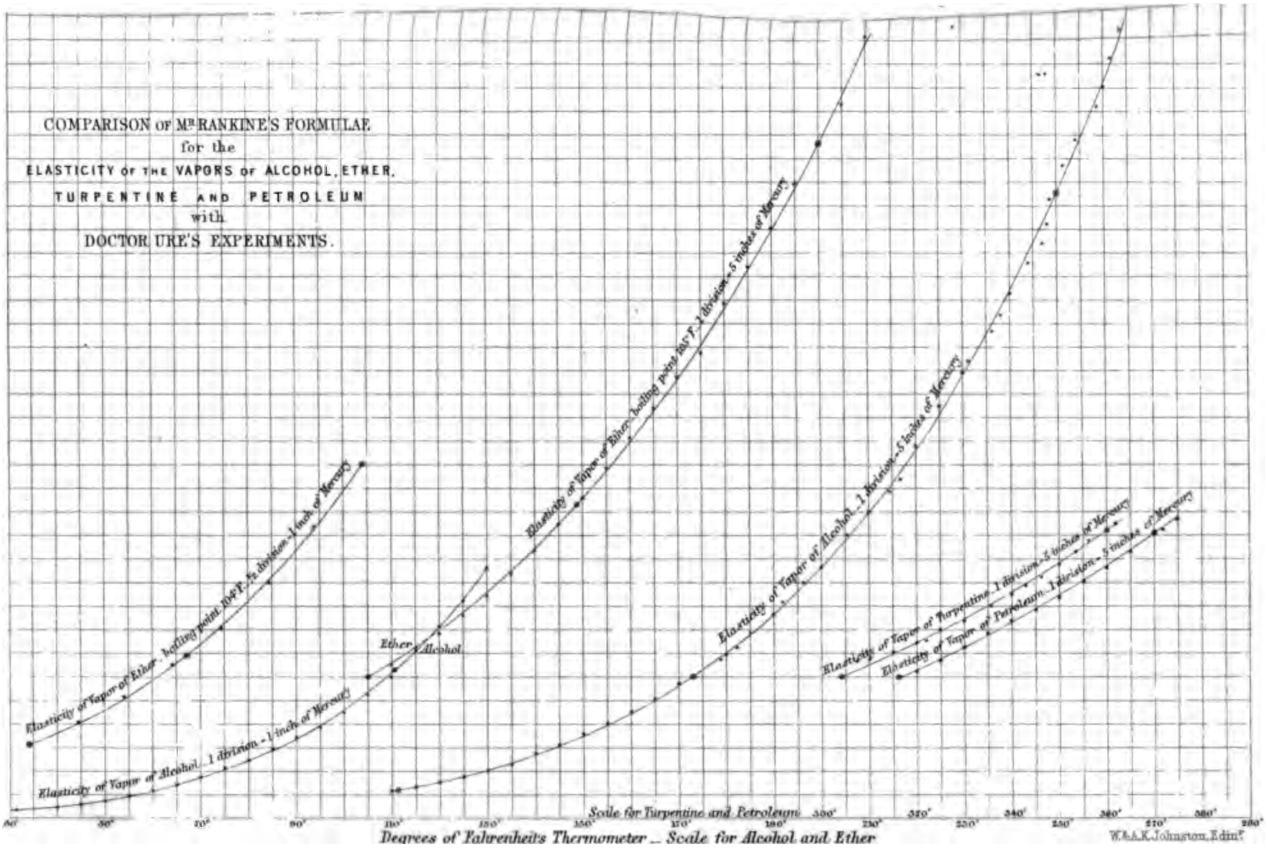
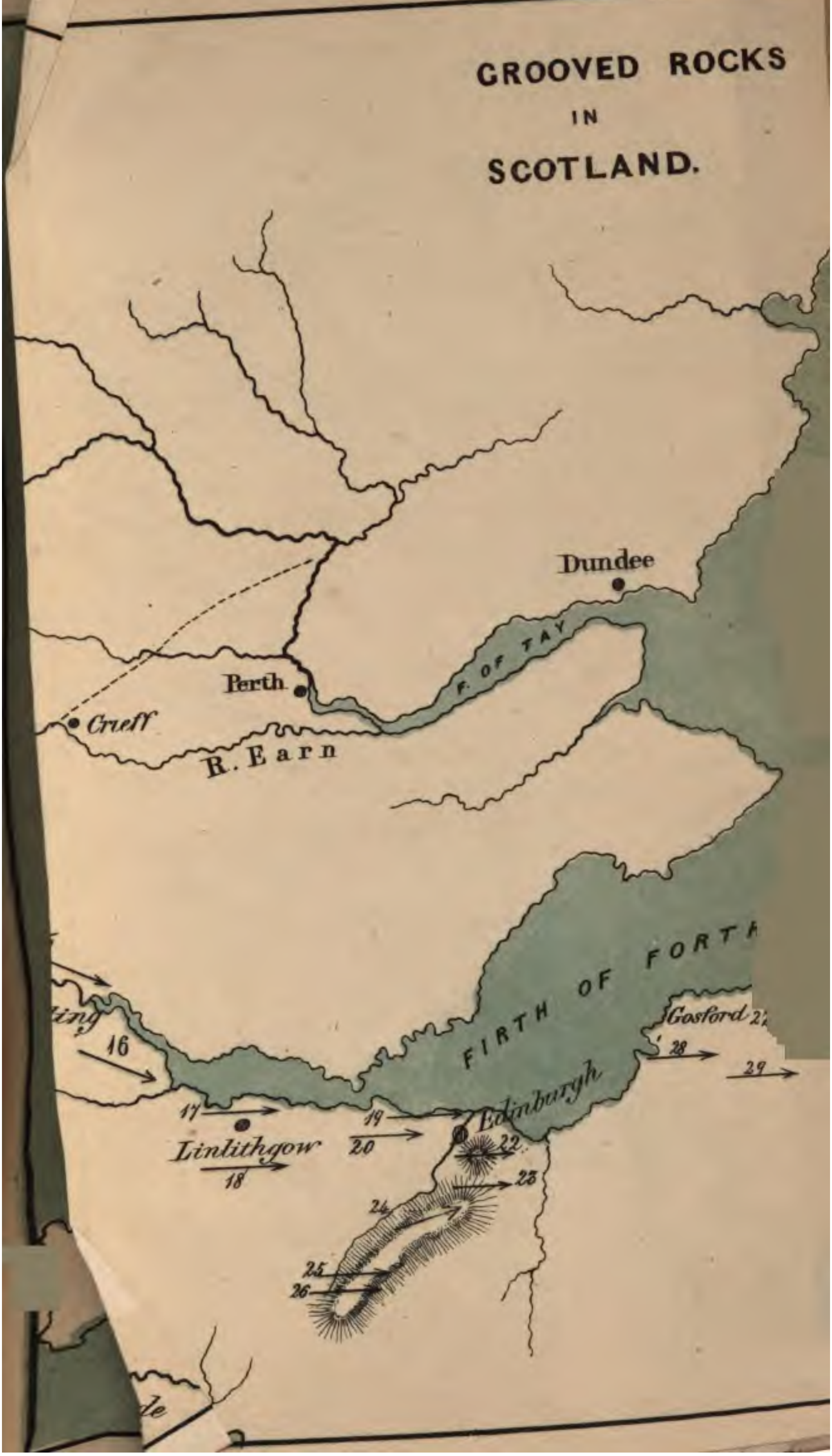


COMPARISON OF Mr. RANKINE'S FORMULAE
for the
ELASTICITY OF THE VAPORS OF ALCOHOL, ETHER,
TURPENTINE AND PETROLEUM
with
DOCTOR URE'S EXPERIMENTS.

Edinr. New Phil. Jour. Plate II Vol XLVII p. 165



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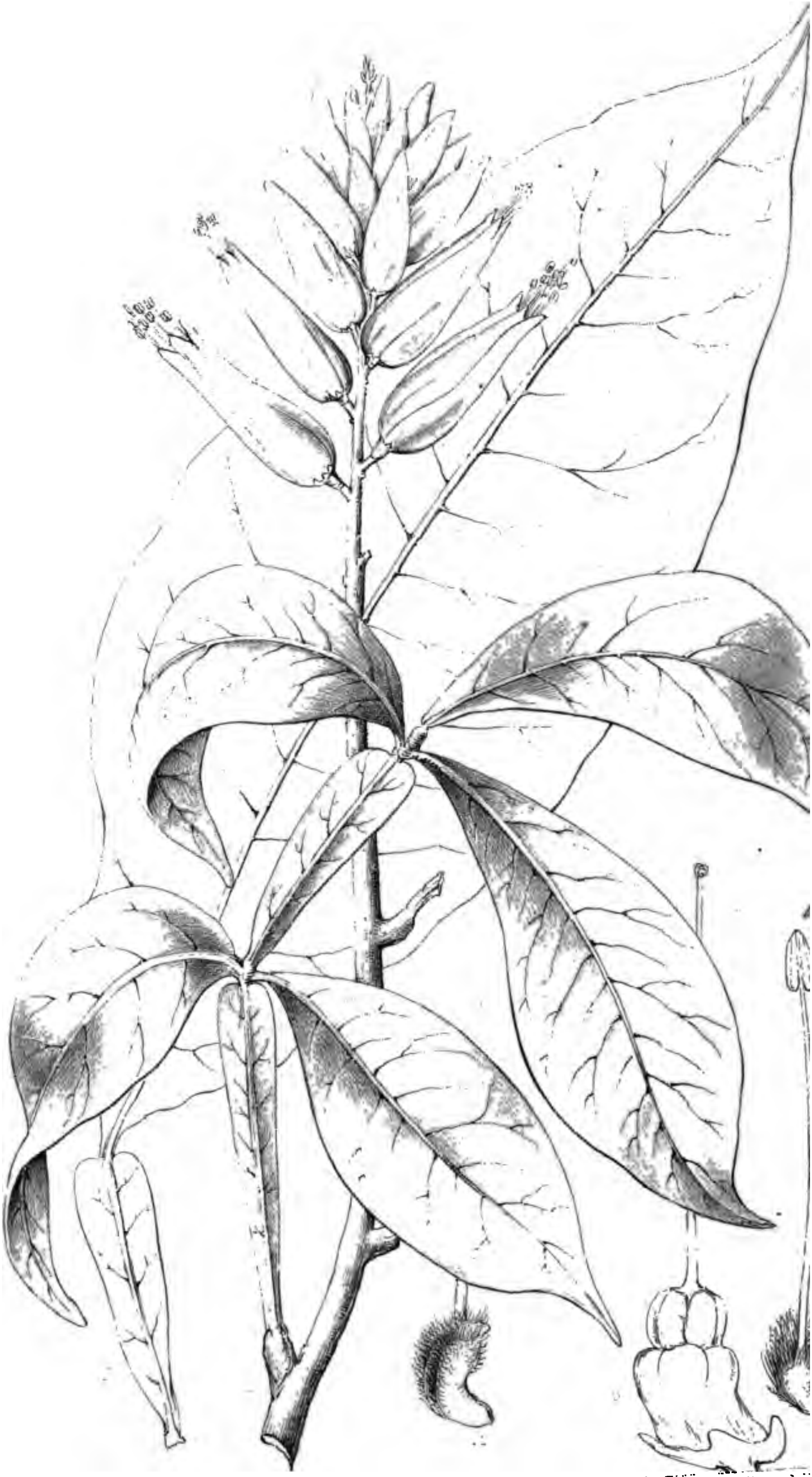

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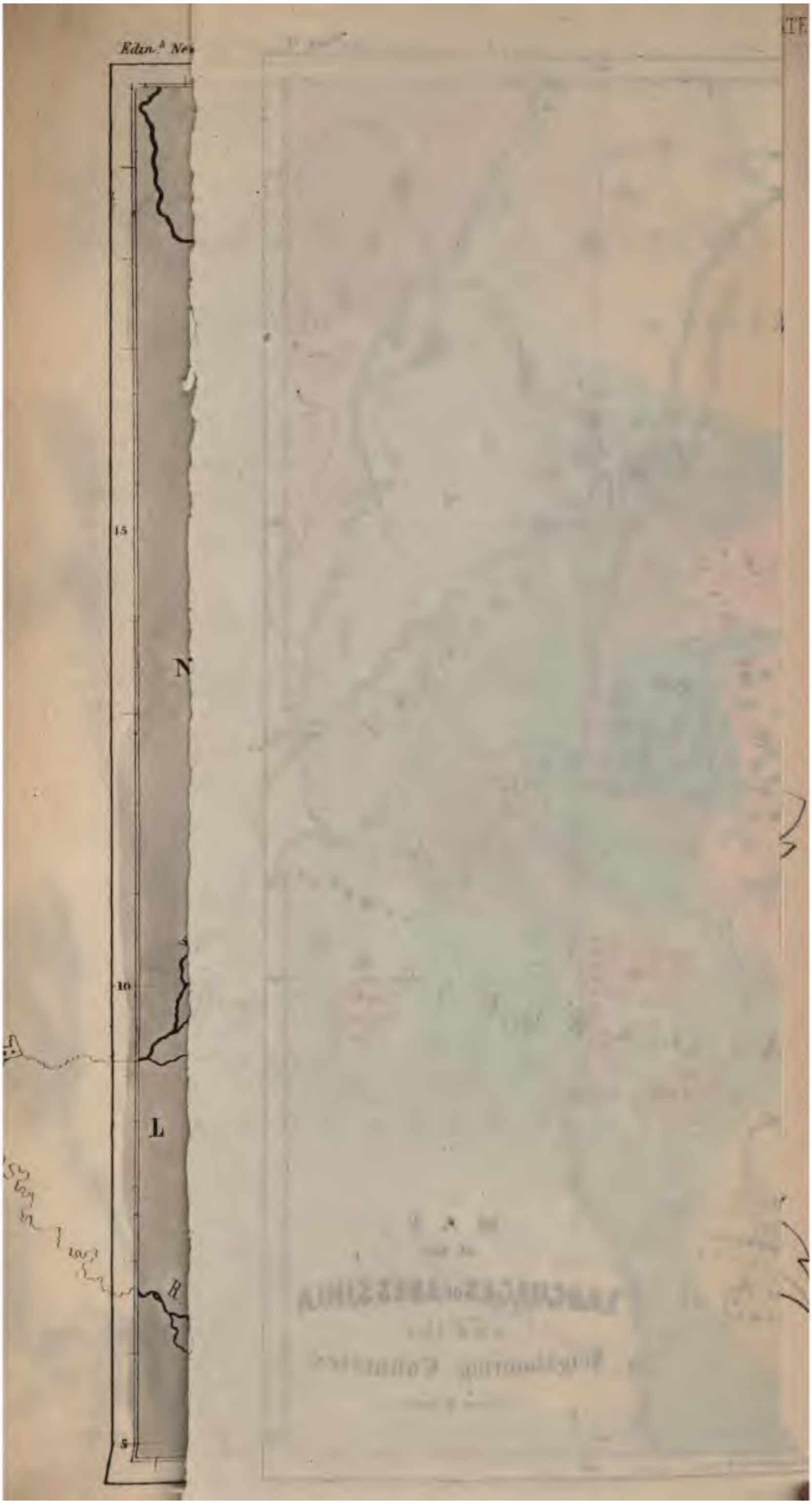
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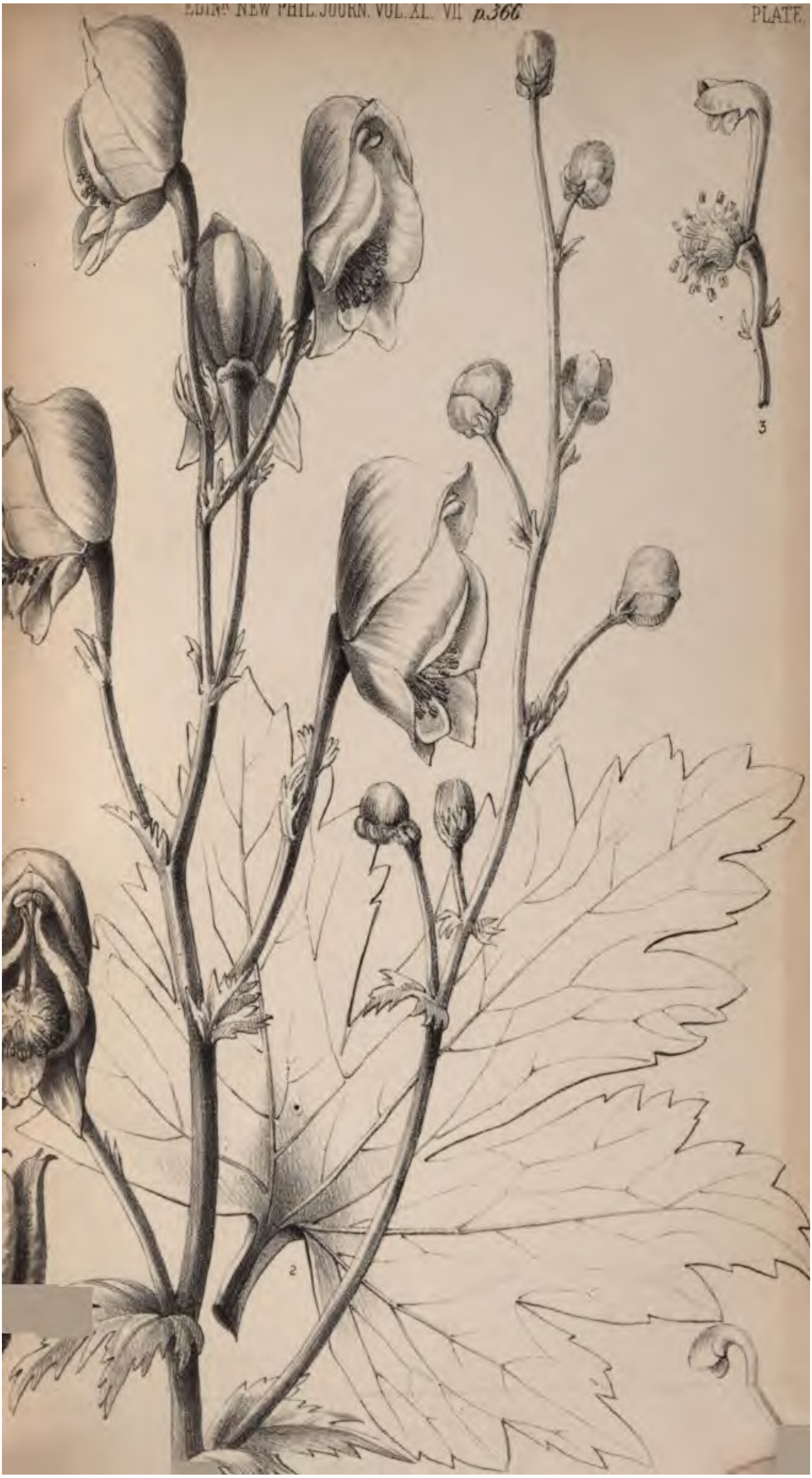

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